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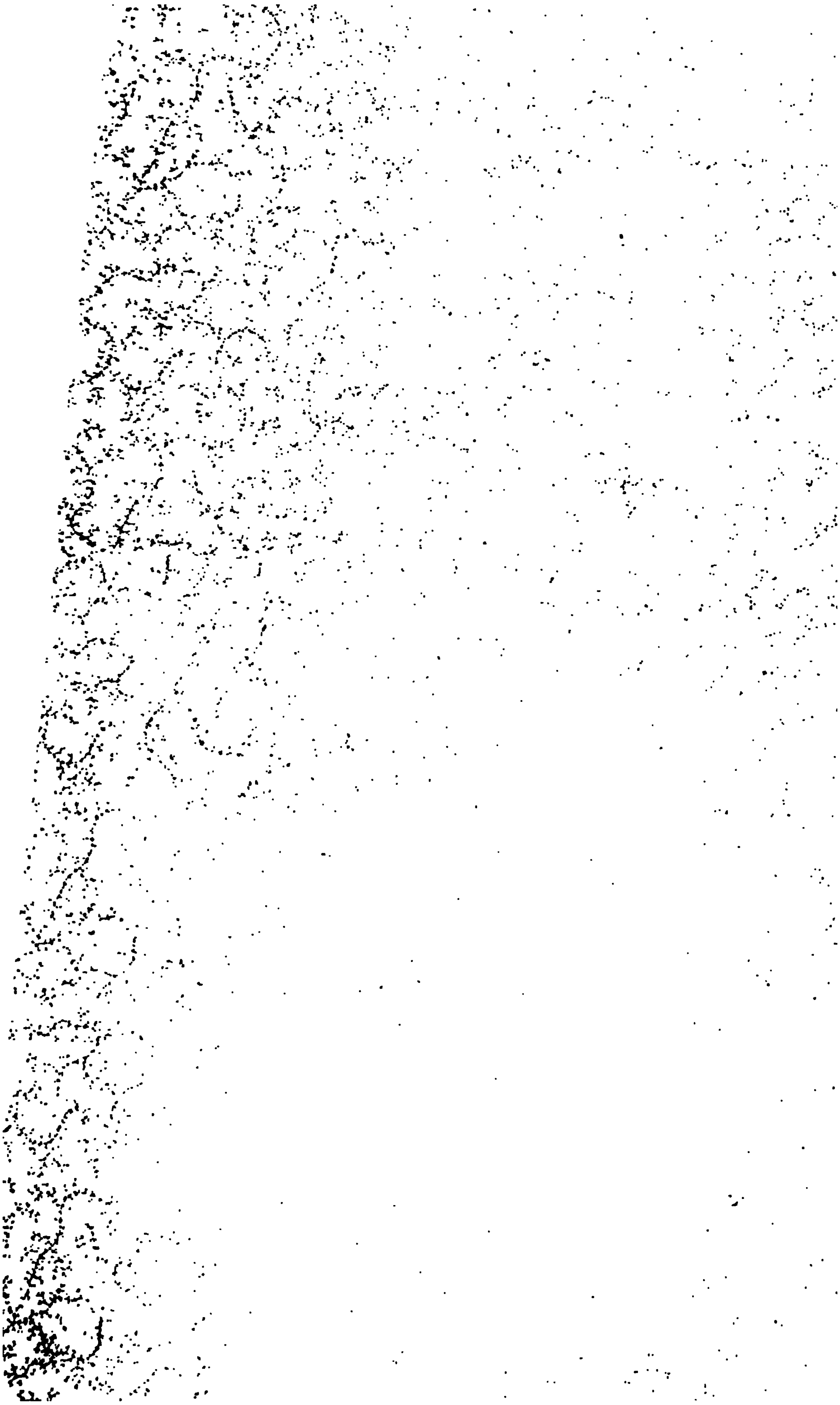

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PERSONAL EQUATION.

E. C. SANFORD.

The terms "personal equation" and "personal difference" are somewhat loosely used by astronomers to indicate such systematic errors in observation as originate in the observer, in distinction from those that arise from instrumental and atmospheric conditions. But the errors thus grouped together in their place of origin have by no means the same causes. Some are purely anatomical, such as the constant and clear difference which has been found between observers in setting the cross-wires of a microscope on the division mark of a scale,[1] or in bringing a star midway between two parallel wires, the cause of which seems to be asymmetry of the halves of the eye. To the same general class would belong astigmatism and other structural defects of the eye as far as they interfere with observation, and color blindness (if that be an anatomical defect), which has been suggested as explaining the different magnitudes assigned by different observers to the same celestial object. Another set are in part from psychic causes. Such are those that beset observations where judgments of time or space must be made. And others are purely psychic, without physical admixture, like the bias for or against special tenths of a second shown in the recorded observations of some astronomers and recognized more or less consciously by others in themselves. It is, however, a portion of those of the second class that were first noticed, first received the name of personal equation, have since received the most careful investigation, and yet remain the most important. Of the discovery and investigation of these it is the purpose of this paper to give an account.

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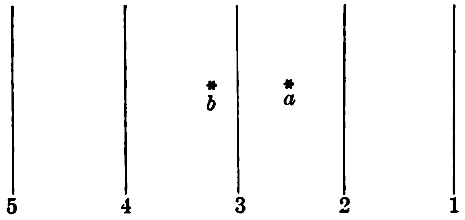
[1] For example, on the limb of a transit circle, or in microscopically comparing standards of length.

Every observatory has for one of its chief businesses the fixing of the instant in which heavenly bodies cross its meridian. On this depends the keeping of the true time, and, in connection with the measurement of the distance of these bodies north or south of the equator, the fixing of their positions and motions in the heavens. And in this very process the personal equation is involved. The instrument used for these observations is, in its lowest terms, a telescope mounted on an east and west axis and turning in the plane of the meridian. In the focus of its eye-piece is a set of fine parallel wires or spider-lines from five to twenty-five in number, called a reticle. The middle one of these lies in the meridian. As the image of the star moves across the field, the instant of its bisection by each of these wires is taken, and the average of the times, provided the intervals between the wires are equal, gives the time of the bisection by the central wire with much less liability to accidental error than if that had been used alone.

At the time of the first notice of personal equation, the method of fixing the instant when the star crossed a wire of the reticle was that of Bradley, or, as it is called, the "eye and ear" method. When the star is about to make its transit, the observer reads off the time from his clock, and then while he watches the star in the telescope, continues to count the second beats. He fixes firmly in mind (as the moving image approaches the wire) its place at the last beat before it crosses the wire and its place at the first beat after, and from the distances of these two points from the wire, estimates by eye the time of the crossing in tenths of a second. A glance at the figure will make the *modus operandi* clear.



The star in most telescopes appears to move from right to left. If we suppose it to be at *a* when the eighth second is counted, and at *b* when the ninth is counted, the time of crossing the third wire will be so many hours, so many minutes, 8.7 seconds. The rôle of the mind in observations by this method is the fixing of the exact place of the star at the first beat, the holding of the same in memory, the fixing of the place at the second beat, the comparison of the two, and the expression of their relation in tenths. When instantaneous occurrences like heliotrope or powder signals or the occultation or emergences of stars are to be observed, several ways are open, but the most common ones require the estimation of the fractional part of the second directly by ear. A few astronomers also were accustomed to observe transits in the same way, treating the passage of the wire like an occultation. But this was generally regarded as a vicious aberration from the true method. The psychic action here is a comparison of the two very short intervals of time between the event and the preceding and following clock-beats; or, regarding the whole series of beats, the interpolation of the sudden sensation into their recurring series.

The "eye and ear" method remained the accepted one till about 1850, and is even now more or less used, especially for slow-moving stars like the pole-star. About 1850 the chronographic method of observation was introduced. The chronograph consists essentially in an evenly revolving drum, with which a writing apparatus, under control of an electro-magnet, is connected in such a way that as the drum revolves the apparatus moves slowly from one end of it to the other. If it were undisturbed the pen would trace a spiral line upon the paper with which the drum is covered. But a clock is brought into the circuit with the electro-magnet, and at each second-beat sends a current through it; the magnet draws back the pen and puts a jag in the line for every second except the sixtieth, which is omitted to indicate the minute. A key in the hands of the observer enables him to record his observation by a jag in the same line or a parallel one. All that remains to do then is to indicate the time on the clock to which a certain one of the second-jags corresponds, and there is a permanent record from which the time of the observation can be read off with ease to a small fraction of a second. By this method of recording the process of observation is much simplified. The astronomer now watches till he sees the star bisected by the wire, then taps his key. He has simply to perceive an event and to will a movement of his finger. The part which the mind plays is thus nearly the same in the observation of transits and sudden phenomena. There is, however, here also a variant application of the method little to be commended. Some observers aim to tap the key so that they shall hear the click of it at the instant of the bisection. They thus add an element of judgment to simple perception and the willing of movement; for to accomplish what they intend, the impulse of will must be given before the star is really behind the wire, and the length of time by which the impulse must precede must vary with the apparent rate of the star. For sudden occurrences they are obliged, of course, to observe like other people.

Now, in all the methods of observation which have been mentioned, observers habitually vary both from the true time and from each other. Their variations from the true time are called their *absolute personal equations;* their mutual differences are their *relative personal equations.* It is natural that the latter should have been first discovered.

So much of a preface has seemed necessary to show what personal equation is. In what follows I propose first to give a brief historical account of the discovery and chief general studies on personal equation, then a more detailed presentation of the circumstances which produce variation in its amount, and, lastly, something of the theories which have been put forward in explanation of it.

THE DISCOVERY OF PERSONAL EQUATION.

The first record of a persistent personal difference between the observations of experienced astronomers goes back a little less than a hundred years. About 1795, Maskelyne, the British Astronomer Royal, noticed such a difference between those of himself and his assistant. At the end of the third volume of the Greenwich Observations he writes as follows:

"I think it necessary to mention that my assistant, Mr. David Kinnebrook, who had observed the transits of stars and planets very well in agreement with me all the year 1794, and for a great part of the present year, began from the beginning of August last to set them down half a second of time later than he should do according to my observations; and, in January of the succeeding year, 1796, he increased his error to eight tenths of a second. As he had unfortunately continued a considerable time in this error before I noticed it, and did not seem to me likely ever to get over it and return to a right method of observing, therefore, though with reluctance, as he was a diligent and useful assistant to me in other respects, I parted with him.

"The error was discovered from the daily rate of the clock deduced from a star observed on one of two days by him and on the other by myself, coming out different to what it did from another star observed both days by the same person, either him or myself . . .

"I cannot persuade myself that my late assistant continued in the use of this excellent method (Bradley's) of observing, but rather suppose he fell into some irregular and confused method of his own, as I do not see how he could have otherwise committed such gross errors."

To the unastronomical mind a difference of eight tenths of a second seems small, but its real signification is more apparent when it is multiplied by fifteen, to give seconds of arc.

For the next twenty years this germ of a discovery lay dormant. But in 1816, von Lindenau mentioned the incident in a history of the Observatory of Greenwich in the *Zeitschrift für Astronomie*, and there it fell under the eye of Bessel, the celebrated Königsberg astronomer. Later, the English Board of Longitude sent the latter a copy of Maskelyne's observations, from which he got a more complete knowledge of the facts. The case impressed him. Considering the easy conditions of such observations with good instruments, and that such were regarded as sure to one tenth or at most two tenths of a second, a difference of eight tenths seemed wellnigh incredible. Its continuance, too, in spite of the desire that Kinnebrook must have felt to bring his observations into harmony with those of his superior, went to prove it involuntary, and therefore important alike to astronomy and anthropology. Bessel desired to know whether such a difference could be found between other pairs of astronomers, and in 1819, while on a visit to Encke and von Lindenau at the Observatory of Seeberg near Gotha, he proposed to test the point with them. Each observed the culmination of several stars, but no second clear night during his stay allowed them to complete the comparison, and the question remained unanswered.

In the winter of 1820–1, at Königsberg, he returned to the subject and made comparisons with Dr. Walbeck, by transits observed on the meridian circle of the observatory. They observed ten stars near the equator on several nights, each observing five a night, and alternating in such a way that those observed by Walbeck on one night were observed by Bessel the next, and *vice versa.* In this way they arrived at determinations of the rate of the clock which should differ by double the amount of the personal difference, and were thus well calculated to show it if any existed.[1] They found that Bessel was always in advance :

	s.
Dec. 16 and 17	1.145
" 17 " 19	0.985
" 19 " 20	1.010
" 20 " 22	1.025
In the mean,	1.041

This result Bessel considers exact within a few hundredths of a second. The difference was striking on the second day and led naturally to redoubled efforts for accuracy. "We ended the observations," says the astronomer, "with the conviction that it would be impossible for either to observe differently, even by only a single tenth of a second."

Later he repeated the experiment with Argelander, using a little different method. In 1821 he observed seven stars in Gemini, each six times, under favorable circumstances, and their mean position for 1820 was calculated. On two evenings in March and April, 1823, Argelander observed the same stars, while Bessel himself determined the clock corrections. The resulting right ascensions were in excess of those previously found by Bessel, and in excess of what they would have been if Argelander had observed the clock stars himself; that is, the stars appeared to Argelander to cross the meridian later than they did to Bessel. The mean difference for the day in March was 1.222 s., for the day in April 1.224, whence B — A = — 1.223 s.[1]

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[1] Suppose the clock to be gaining and that Bessel observes earlier than Walbeck. Then for the stars which Bessel observes first and Walbeck second, the clock rate found will be the real gain of the clock plus the difference of the observers. When Walbeck observes first and Bessel second, the rate found will be the gain of the clock minus the difference of the observers. The difference to the two rates of gain found will be double the personal difference. If the clock is losing, the case is similar.

Bessel, however, was not content here; Walbeck and Argelander were less practiced in transit observations than he, and he thought that possibly the cause of the difference lay in this. He accordingly asked Struve, of Dorpat, to compare observations with him by means of comparisons with Walbeck and Argelander as they passed through his city. In 1821 Walbeck and Struve observed together on four days, with the resulting equation:

$$S - W = -0.242 \text{ s.}$$

whence $B - S = -0.799$

In July, 1823, Argelander obtained the following:

$$S - A = -0.202 \text{ s.}$$

whence $B - S = -1.021$

The personal difference, therefore, did not originate in difference in practice.

There is, however, a difference of 0.222 s. between the two values for B — S, and, since there is very little uncertainty in the individual determinations, is evidence of change in one or another of the four observers; most probably in B or S, for the comparison of the intermediary with S was made each time soon after that with B. A single direct comparison points the same way. In October, ~~1814~~ 1824, Struve visited Bessel, and the two observed together ; Struve observing the transit of one star, Bessel of two. From these by calculation the equation B — S = — 0.044 s. is found, and though it rests on a single transit, is not without weight, for Struve considered the observation successful, and the agreement of the single wires testifies the same. At any rate the error was not one of eight tenths of a second.

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[1] The statement of the personal difference in this form has led to its being called the "personal *equation*."

This is sufficient to establish the variability of the personal equation ; but later comparisons (leaving Bessel's first study for the moment) give further evidence of the same thing. In 1825 the visit at Königsberg of another astronomer, Knorre, who had just compared with Struve, gave opportunity for repeating the determination of B — S. The result was, B — S = — 0.891 s. A direct comparison in 1834 gave B — S = — 0.77 s. Taking all together we have :

		s.		
1814,	B—S	= — 0.044,	direct	comparison.
1821		= — 0.799,	indirect	"
1823		= — 1.021	"	"
1825		= — 0.891	"	"
1834		= — 0.770	direct	"

Bessel's next thought after having established the fact of a personal difference, was to find its cause. To that end he began to vary the conditions. He first substituted the sudden disappearance or reappearance of a star, as in occultations and emergences, for its steady motion across the reticle. Seventy-eight comparisons of this kind gave for Bessel and Argelander, B′ — A′ = — 0.222 s.; another set of twenty-one gave B′ — A′ = — 0.289 s. A comparison of Struve and Argelander on these sudden phenomena developed no significant personal difference. Observations of this kind are less certain than transit observations, but they seemed to Bessel to indicate that the trouble lay in combining the steady advance of the star with the sudden beat of the clock, and his next experiment was therefore with a variation in the clock. On two nights he observed a chosen series of stars with a clock beating half seconds, with the following result (indicating by B″ his observations with the half-second clock):

s.
On the first night $B - B'' = -0.520$
" " second " $B - B'' = -0.467$

That is, he observed transits later, on the average, by about half a second in this way than with the whole-second clock. Argelander's observations on the half-second clock compared with those of Bessel made in the ordinary way showed no particular change: $B - A'' = -1.246$ s., or, in another series, $B - A'' = -1.208$ s.

Observations with a half-second clock at Dorpat gave $S'' - A'' = -0.227$; from all of which it appears that Bessel alone had his personal equation changed by the alteration of the rapidity of the beat. One other point the Königsberg astronomer investigated, namely, the effect of the apparent rate of the star, which varies with its declination, on the personal equation. This is of great importance, for if it be found that the rate has no effect, then, provided the personal equation is constant for the time being, it will affect equally the times of transit of all stars observed by the same observer, and will not change at all their relative times of transit, on which their right ascensions depend. Bessel varied the apparent rate of motion by the use of different powers in his eye-piece, and concluded that the rate had no influence, at least for differences equal to those from the equator to within 30° of the pole.

In brief, Bessel established these points: the fact of personal equation, its spontaneous variation in considerable periods of time, and its artificial change, for himself at least, with change of the clock beat and from transits to sudden phenomena, and he tried, with negative results, the influence of the rate of motion. How important these discoveries are in relation to present knowledge will appear as the narrative proceeds.

Bessel's theory of the psychical cause of the personal equation which he had discovered will be considered elsewhere. In brief it is that the work of the mind is the comparison or superposing of the unlike impressions on the eye and ear, and that observers differ in the readiness with which they accomplish this; an additional difference coming in when one of them goes over from seeing to hearing and the other from hearing to seeing.

It has more than once been noticed as a fortunate coincidence for the knowledge of this matter, that the discoverer of the personal equation should himself have had so large a one; and such it probably was. But its very size has provoked incredulity. It seems simply impossible that two practiced astronomers should observe the transit of a star differently with a clock beating seconds by almost a beat and a quarter. Encke has contended, and after him Wolf, that Bessel must have differed from other observers in the counting of his seconds; counting, for example, a transit which occurred seven tenths of a second after the pointer of the clock had passed the fourteenth division of the face, as 13.7 seconds instead of 14.7 with other astronomers; in other words, adding the fractional part to the second completed instead of the second begun.[1] The strongest argument in support of this view is that furnished by Bessel's own experiment with the half-second clock, where the average of the two equations obtained gives B — B″ = — 0.49 s., showing that he observed later by a half second when he timed the transit by half seconds—exactly what would occur if it was his counting that was at fault. And Wolf cites as further evidence the case of a Parisian observer whose results differed from those of his colleagues by a whole second, until the matter was forcibly brought to his attention by setting him to observe the disappearance of a moving object behind an obstacle while he counted the seconds out loud and some one else marked the instant of phenomenon for him by a blow on the back. And there have been other instances of the same kind.

On the other hand, Peters shows that the personal equation between Bessel and Argelander is not unique, as it should be if the former, as Encke says, had counted his seconds "too early as against all other astronomers," but rather the last term of a series of which lower terms can easily be shown. For example:

s.
Oct. 7, 1833. Nehus—Wolfers = 0.62
" 8, " " = 0.84
1837. Gerling—Nicolai = 0.78
1854. Main—Rogerson = 0.70

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[1] Such a possibility is also explained as follows: The audible beat of the second is made when the pointer is still moving from mark to mark on the dial. A difference of a second would be introduced if one observer associated the beat with the dial mark which is left and the other with the one that is approached.

To these may be added, not counting Maskelyne and Kinnebrook,

1843. Goujon—Mauvais = 0.58
and 1859. Sashoo—Jacobs = 0.80

Again, if the size of Bessel's personal equation had been due to his method of counting the seconds, his observations of sudden phenomena should have shown it as well as his transit observations; whereas the former gives $A' - B' = 0.222$ s., and the latter $A - B = 1.223$ s. That Bessel's method of counting was the same in the two cases, Peters testifies from conversation on the subject with Bessel himself and from observations made by Bessel in his presence. The presumption is also natural that the possibility of a difference in counting must have suggested itself to a mind so fertile as Bessel's. Wolf is right in judging the fact a hard one to explain on either hypothesis, but it seems to me rather less hard on the supposition that the astronomer counted his seconds correctly than on the other.[1]

PERSONAL EQUATION BEFORE THE INVENTION OF THE CHRONOGRAPH.

In the years following Bessel's discovery there were occasional recognitions of personal equation. That of Wolfers and Nehus determined in 1833 has already been given. At an early date, also, Dr. Robinson, of the Observatory of Armagh, noticed a difference of personal equation as the first or second limb of the sun or moon was observed; but not till about 1838 does it seem to have received much consideration in actual practice. In the volume of the Greenwich Observations for that year, Airy, the Astronomer Royal, began to publish the figures for the personal equations of the transit observers under his charge. During the two years previous, differences had been noticed, but were too small to be significant. The figures were not found by special tests, but calculated from the clock errors observed in the routine work of the observatory. In the same year, Gerling, professor in the University of Marburg and director of its observatory, published the result of the measurement of the longitude of Göttingen, Marburg and Mannheim, made in connection with Nicolai, Gauss and others. After the measurements had been completed the observers compared among themselves for personal equation, and made the longitude observations uniform by reducing them all to Gerling's own as a norm. The following are the figures obtained by these comparisons:

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[1] The fact that Bessel observed with Encke and von Lindenau in 1819 without finding any personal equation has been cited as evidence that he counted his seconds correctly. But as has been said, no personal equation was found, not because there may not have been one, but because the comparisons that were to show it could not be completed.

For transits:

Gerling—Goldschmidt $=0\overset{\text{s}}{.}195$ from 26 obs. of stars.
Gerling—Nicolai $=0.783$ " 72 "
" " $=0.681$ from 190 observations of the transits of a spring pendulum.
Gerling—Hartmann $=0.051$ from 180 observations of the transits of a spring pendulum observed with a half-second clock.

For flashes of light:

Gauss—Goldschmidt $=0\overset{\text{s}}{.}088$ from 292 observations.
Gerling—Goldschmidt $=0.027$ " 56 "
Gerling—Nicolai $=0.157$ " 308 "
Gerling—Hartmann $=0.055$ " 267 "

As early as 1842 it occurred to Arago that personal equation might be reduced or abolished by giving the observer but one thing to attend to. On New-year's-day, 1843, he applied his idea in the case of a young astronomer, Goujon, whose personal equation usually reached about half a second. He had Goujon indicate the passage of the star by a quick stroke, while another observer, Bouvard, kept the time and estimated the fraction of a second. The personal difference disappeared. To remove the doubt that the difference might have been due to a slowness of hearing, he caused a third person to give the taps while Goujon and Bouvard took the time together. Again they agreed through the forty trials made. During the same year Arago made further tests with a *chronomètre à pointage* which was so constructed that on the pulling of a trigger the second-hand made a dot on the dial from which the fraction of the second could be read off. The observer had only to pull trigger at the instant of the transit and his record was made. With this instrument, Goujon and Mauvais, who otherwise differed by 0.58 s., observed alike. The limit of accuracy in these comparisons was about one twentieth of a second.

In 1843 and 1844, Otto Struve measured a number of personal equations in connection with the determination of the difference of longitude of Pulkowa and Altona, but the figures are of no particular consequence to the subject in hand.

THE INVENTION AND ADVANTAGES OF THE CHRONOGRAPH.

The first attempt at the simplification of transit observations by reducing them to the indication of the instant of passage, and the first suggestion of the chronographic method, antedates the experiments of Arago just recounted by fourteen years. In 1828, J. G. Repsold, director of the Observatory of Hamburg and celebrated mechanician, proposed an apparatus in which the record was taken by means of a point connected with a key on a strip of paper regularly moved by clock-work. In taking a transit observation, the machine being in motion, the observer was first to record a beat of the clock by a tap of the key just before the star began to cross the reticle, then in the same way the crossing of each spider-line as it occurred, and finally the stroke of the clock following the crossing of the last one. From the distances of these dots the time was to be measured off. It was essential, of course, that the strip should move evenly from the first dot to the last, about three minutes for equatorial stars. But the apparatus first made being without a governor, failed in this particular, and the death of the inventor prevented the perfecting of the instrument.

The chronographic method, as it is now practised, is an American product—so distinctly so that it is frequently called "the American method." Of its origin Professor B. Pierce speaks as follows: "The American method is the unquestionable product of the Coast Survey of the United States, and was the legitimate result of the rigid and profound methods of research which are uniformly adopted in this magnificent work. The first conception was in the mind of the superintendent himself, Professor Bache, and its complete development and ultimate success were owing to the united action of Professor Bache and his friend and assistant, Mr. Sears C. Walker. The details of the instrumental invention and execution were intrusted to Messrs. Saxton, Bond, Mitchel and Locke. Different plans were proposed, but that of Mr. Bond is the one which is at present [1860] adopted in the Coast Survey."

The principle of the chronograph has already been described, and it would be aside from the subject in hand to notice the variations in detail which have been introduced. Suffice it to say here that those in most common use to-day show variations in detail only.[1]

In 1851 the Bond chronograph was exhibited at the meeting of the British Association, and in 1854 the method was introduced at Greenwich. From time to time other observatories have followed and the method is now the accepted one.

The adoption of the chronograph did not do away with personal equation, but it greatly reduced it. Out of thirty-four personal equations determined at Greenwich from 1854 to 1856, only four exceeded 0.1 s., and the highest was 0.17 s.; but in the three years previous, by the old method, out of thirty-three, nineteen exceeded 0.1 s. and eight were over 0.17 s. The difference is due in part to a change of observers, but is nevertheless significant. From an astronomical point of view, however, the increased certainty of the observations is of far greater importance than the lessening of the amount of personal equation. Dunkin found from a comparative study of the observations made on the Greenwich transit instrument in the last year of the "eye and ear" method (1853), with those on the same instrument in 1857, that the probable error of an observation at a single wire by the "eye and ear" method was ±0.074 s., the probable error of a complete transit ±0.028 s.; the probable error of an observation at a single wire by the chronographic method was ±0.051 s., that of a complete transit ±0.017 s. Approaching the same question again in 1864 from another point of view, he arrives at figures for the probable error of an observation at one wire by the two methods which show the effect of the change on individual observers:

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[1] Chronographs have been devised which should give the instant of an observation in printed figures, but they have not, I believe, yet reached perfect action.

	Henry.	Dunkin.	Ellis.	Various observers, mostly less practised.
Eye and ear,	±0.112 s.	±0.062 s.	±0.069 s.	±0.089 s.
Chronograph,	±0.058 s.	±0.048 s.	±0.053 s.	±0.060 s.

Other advantages are credited by Dunkin and others to the chronographic method, but among these the point of special interest in this connection is that the personal equation seems less variable in its amount.

WAYS OF DETERMINING THE AMOUNT OF PERSONAL EQUATION AND DEVICES FOR EXCLUDING IT IN OBSERVATION.

The reduced personal equation that persisted in spite of the chronograph was still, in the eyes of astronomers, a blemish on the fine accuracy of their science, and from time to time efforts were made for some means, either of determining its amount exactly so that it could be taken into calculation, or of changing the method of observation so as to exclude it. Three ways of determining the amount of personal equation have already been mentioned, that of the Greenwich Observatory, where the custom long has been to get it from the clock corrections found in the routine work of the observatory, and the two ways used by Bessel in comparing with Walbeck and Argelander. Another is the method of divided transits: both observers use the same instrument and observe the same culmination, one observing the passage over the first wires of the reticle, the other over the last, changing the order in which they observe from star to star so as to exclude possible errors in the corrections for the distances of the wires. This was the method in most common use where personal equation was found from special comparisons. Its chief advantage is that all the instrumental conditions are the same for both observers; its chief disadvantage, provided both are equally accustomed to the particular instrument used, is the hurry of changing places, which might prevent the second observer from observing as he would at his leisure. A special "binocular eye-piece" that was designed to avoid this difficulty was tested at Greenwich in 1852 and 1853. An equilateral prism set in the eye-piece gave two views of the transit from positions 120° apart, thus enabling observers to compare without inconveniencing each other. A method of determining personal equations with transits of the limbs of the sun also allowed simultaneous comparison of a number of observers. The images of the sun and the reticle were projected from the telescope on a table or semi-transparent screen, and the transits were observed as they occurred there. There are still other methods, *e. g.* the observation of the same phenomena with adjacent instruments, or the determination of the longitude of points whose distance is already directly known. But there remains one that deserves attention, namely, that of artificial transits. Its advantage is that the phenomenon to be observed can be produced at any time and as often as necessary. Gerling seems to have been the first to get at the personal equation in this way. When he was comparing himself with Goldschmidt and Nicolai, at the suggestion of Gauss, he made use of the transits of an inverted spring pendulum (*Kater'scher Feder-pendel*) in addition to transits of the stars. At an early date Prazmowski used the vibrations of a declination needle for a similar purpose. Such methods are applicable, provided that the personal equation remains the same for the artificial transits as it is for the real, a condition which is probably much better fulfilled in some apparatuses than in others. Against all comparisons by means of real stars, on the other hand, it may be urged that the atmospheric conditions which make a star at one time clear-cut and at another time "woolly," interfere also with the accuracy of the results.

As intermediate between the way of getting rid of personal difference by fixing its amount and allowing for it, and those of excluding it in the observation, two practical devices may be mentioned for avoiding it without knowing its amount. Error is not brought in unless the observations of astronomers between whom such a difference exists are united in computation. This is guarded against by indicating with each observation by whom it was taken. In observations for longitude, however, the combination of the work of two observers is a matter of necessity, and here it is customary for them to exchange stations. Both plans, it will be seen, assume that the personal equation remains practically constant, a thing that seems to be sometimes true and sometimes not.

The devices for excluding personal equation at the moment of observation aim to carry the simplification beyond the point reached by the chronograph. That left the observer free to concentrate his attention on the star; these do away with the motion of the star;

and one even goes so far as to do away with the observer himself. The way in which the first is accomplished is by giving to the whole instrument,[1] or to the reticle,[2] a motion equal to that of the image of the star. This allows the observer, since the motion is under his control, to bisect the image with a line of the reticle as exactly as if both were at rest. When the bisection has been accurately made, the position of the instrument at a certain instant and the time are recorded by the observer or automatically, and from the record the time of the transit of the meridian is calculated. Another means to the same end is instantaneous illumination of the wires.[3] The illuminating flash is made to occur at intervals exactly equal to the time required by the image of the star to move from wire to wire, and its occurrence is recorded, together with the beats of the clock, on the chronograph. The beginning of the series of flashes is under the control of the observer, and is made by him to coincide exactly with a bisection, three or four trials generally being needed; after this the flash repeats itself and its record on the chronograph at each bisection. The rate at which the flashes recur is also adjustable to the declination of the star. The instantaneousness of the flash makes the image to all intents stationary at the instant of bisection.

The difficulty with these methods is the complex apparatus which they require. An instrument of this kind, to be of any service, must be adjustable through a considerable range to the apparent rate of the stars.

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[1] Liais.

[2] Radier and C. Braun. Suggestions for something of the same nature were made by Wheatstone, and, I believe, by A. S. Herschel.

[3] Langley.

The proposal to exclude the observer himself comes from M. Faye. He suggested the substitution of a sensitive plate for the eye of the observer, and the instantaneous photographing of the wires and the image, the instant of the exposure of the plate being recorded electrically. This answers best for transits of the sun where there is plenty of light, but is not impossible for stars. Professor Langley thinks "it is perhaps not too much to say that it will probably be the method of the future."

INVESTIGATIONS OF THE ABSOLUTE PERSONAL EQUATION.

So far the relative personal equation alone has been spoken of. As long as this alone had been measured, astronomers could be told that though they knew how much they differed among themselves, not one of them knew how much he differed from the truth. They were therefore naturally curious to know what their differences from this were—in other words, what their absolute personal equations were. And the question had besides interesting ramifications into physiology, psychology, and anthropology. Artificial transits and electrical appliances for recording already in use gave the means required for these measurements, and they were soon begun. In 1854, Prazmowski suggested an apparatus for this purpose. It was to consist of a disk carrying a luminous point for a star, and closing an electric circuit the instant the image of the star was bisected by a line of the telescope through which the transit was observed. The second, the instant of observation, and the true time of the transit were to be recorded by electrical means on a moving strip of paper. By varying the distance of the telescope and the rate of the disk, the conditions of actual observation as regards power of the instrument, rate of the star, etc., could be paralleled. The apparatus could be used for observations by "eye and ear," by taking the seconds from the click of the electro-magnet that recorded the seconds on the strip.

In 1856, Professor Mitchel announced an apparatus for the measurement of absolute personal equation. Two years later he communicated to the English Astronomer Royal the result of a series of experiments on the subject. He used ten artificial stars attached to a revolving disk, recording their real transits electrically, while the observer did the same in a similar way for their observed transits. These records corrected for the errors of the apparatus give the absolute personal equation, or, as Mitchel calls it, the "absolute personality of the eye." The "personality of the eye" he measured both for transits and for the perception of a white stripe on a dark ground; the "personality of the ear" and "personality of touch" likewise by stimuli suited to those organs. He and his assistant, Twitchell, made daily observations for sixty or seventy days, and about thirty persons besides themselves were tested. The following figures are the means of two hundred and fifty-five observations each, the eye stimulus being the white stripe and the ear stimulus a quick tap:

		Mean. s.	Minimum. s.	Maximum. s.
M.	Eye,	0.161	0.139	0.191
	Ear,	0.164	0.143	0.193
T.	Eye,	0.144	0.118	0.184
	Ear,	0.153	0.129	0.201

Special tests were made to find whether the eye and ear were constant in their "personality" for short periods of time. Mitchel and his assistant on several days took sets of ten observations each in alternate minutes, and found the eye personality liable to variations of as much as 0.020 s. between the sets of ten. Touch gave results similar to those for the eye, and experiments were not continued.

In taking the artificial transits, Mitchel found that he himself, his assistant, and all the persons tested anticipated the true time. For himself this anticipation was on several occasions as great as 0.1 s. on a mean of ten, and showed somewhat of a daily variation. This led to the trial of artificial emergences and immergences. The first gave results like the simple observation of the white stripe; the second showed the tendency to anticipation and less steadiness. To put what he had discovered to practical use, he replaced the spider-lines in the reticle of his instrument, except the central one, with occultating bars, and observed by immergences, emergences and transits of the central line, but the effect of the change he was not able at the time to report.

In the same year, 1858, Julius Hartmann, Professor in the Lyceum at Rinteln, also published the description of an apparatus for the same purpose, and the results of a study made by means of it. His apparatus consisted of a horizontal clock controlled by a conical pendulum, the regularity of which was tested by a siren. A wheel carrying a three-inch disk of paper was made so as to shunt in or out of the clock system, and could be set so as to produce at any fixed hundredth of a second a sudden flash through a little hole in the disk, or the transit behind a white thread of a steel bead on the surface of the disk. These were to be observed by the "eye and ear" method, the clock itself giving the second-beats. The conditions could be varied by changing the distance of the bead from the centre of the disk and by changing the distance of the observer. The maximum error of the machine was not more than 0.03 s. or 0.04 s.

In using his apparatus, Hartmann was accustomed, when once the disk was set at any fraction of a second, to let the phenomenon to be observed recur again and again at periods of eight seconds (for light flashes at first even every second) till it could be observed, as it were, at leisure. The result was often a considerable difference in the answers made at the beginning and end of the process. In this way, in his opinion, the observer quickly got an observation free of surprise.

The interest of these repetition experiments is perhaps other than the experimenter realized. Not only is surprise avoided, as he supposed, but the nature of the psychic process is changed. An observer soon catches the rhythm of such a recurring series, and as each member of the series comes, it finds the mind in a state of active expectation. As experimenters in the psychological field have since shown, the reaction time for an expected stimulus is very much abbreviated—so much so, indeed, that the reaction may even precede the stimulus which it should follow. Perception does not then lag behind sensation; the inner or mental series is pushed forward in expectation and synchronized with the outer actual series of stimuli.

The figures for the personal equation found by these experiments are very small, and the mean error for a single observation, since the observer was sometimes ahead and sometimes behind the true time, frequently, if not always, exceeds the average personal equation found.

In observing transits of the bead when it moved along a scale divided to tenths of a second, Hartmann noticed an interesting illusion. Sometimes when he knew beforehand the exact place where the star should be at the second stroke, he seemed to see it from 0.03 s. to 0.08 s. in advance of its true place. With particular effort to see exactly and extreme attention, the star seemed to stand still an instant at the place where it was when the stroke entered. At other times it seemed to advance steadily and was in motion in its right place at the stroke. This happened most frequently at the end of a series of observations or when the experimenter observed somewhat nonchalantly. He does not venture an explanation, but suggests that the differences may be caused by differences of attention; the star being most regarded in the first and third and the clock-beats in the second. Something similar he thinks possibly happens with the flashes of light, though he was not able with his apparatus to demonstrate it.

The conclusions to which this experimenter comes are:

First, that the absolute personal equation, when it amounts to a tenth of a second or more, is not necessarily grounded in the make-up of the eye or ear or in the mind; and that the "reception time" taken alone rarely rises above a few hundredths of a second. Second, that it finds its cause rather in unequal attention, surprise, defective memory of the series of light and sound impressions, wrong customs of observing, etc.—all of which may perhaps be helped by a knowedge of the error. Third, that it is not constant, but varies from day to day and from series to series, and even with the tenth of the second in which the phenomenon happens to fall. A tendency to this last variation Hartmann thinks he is able to find also in the observations of Gauss and Goldschmidt on heliotrope and powder signals in the longitude determination before mentioned.

In 1863, F. Kaiser, of the Observatory of Leyden, published a method of getting the absolute personal equation quite different in the manner of measuring the time from those generally employed. It depends on the observation of the coincidence of the beats of two clocks beating at slightly different rates. An example will show how the thing is done. Suppose the observing clock beats forty-nine times to fifty beats of the standard clock, that is, beats every 1.02 s. At the occurrence of some phenomenon the pendulum of the observing clock is released, and its beats are counted till they reach a coincidence with those of the standard clock. If thirty-five beats are counted, and at the coincidence the standard clock reads 10 h. 42 m. 50 s., the true time of the phenomenon is gotten as follows: $35 \times 1.02 = 35.70$ s. as the time from the starting of the observing clock to the coincidence. This taken away from the time indicated by the standard clock leaves 10 h. 42 m. 14.30 s. as the true time of the phenomenon. To apply this to measuring the absolute personal equation for artificial transits, it is only necessary to have the pendulum held by an electro-magnet which shall release it on the breaking of the circuit, and to make that correspond with the bisection of the star. In Kaiser's apparatus the artificial transits were managed by placing at one end of a bar of wood a lamp, and before it a screen with a small hole in it; at the other end of the bar a lens which projected a fine image of the hole on another screen of oil paper where the reticle was represented by a vertical black line. The bar was turned by clock-work, could be varied in speed, and was arranged to break the electric circuit at the instant of the passage of the star. When the absolute personal equation was to be taken, the observer made his estimate of the time of the transit by eye and ear, while an assistant counted the strokes up to a coincidence, and from that, as explained above, the real instant of the transit was found ; the difference of this from the observed time gave the personal equation. The apparatus could be varied to represent occultations and powder-signal flashes, and could be applied to measuring the personal equation in observing by the other method, though for that it was more complicated.

Experiments were made from 1851 to 1859. The following figures are from May of the latter year :

Observer.	Mean personal equation.	Prob. error.	Limits between which the personal equation varied.	
Gussew,	—0.10	±0.057	+0.07	—0.31
Brouwer,	+0.18	±0.095	+0.33	—0.21
Kam,	+0.15	±0.083	+0.29	—0.18
P. J. Kaiser,	+0.08	±0.088	+0.29	—0.11

The accuracy of measurements of the personal equation by this method depends on the accuracy with which a coincidence of the clock-beats can be observed. Later experimenters have shown that such observations are themselves liable to a certain small personal error.

In 1862 he improved upon his former apparatus for artificial stars, and adapted the new one to chronographic recording. In the first months of 1867 the Kaisers (father and son), Kam, and Van Hennekeler made observations to the number of several thousand with this apparatus, from which they concluded that for them the personal equation was small and, provided the circumstances of observation remained the same, constant; that it might be reduced by practice; that the motion of the star, alternately from right to left and left to right, made no difference, except for Van Hennekeler, who observed a little later when the motion was from right to left than when it was the reverse; and that when they observed motions up and down by the introduction of a prism, all were made later.

Another careful measurement was undertaken in 1862 by the Swiss astronomers Hirsch and Plantamour, in connection with the measuring of the difference in longitude between Geneva and Neufchatel. In May, 1861, they determined their relative personal equation by the observation of nine stars, using the chronographic method, and found P—H = — 0.082 s. In October of the same year, with somewhat unfavorable conditions, they found P—H = — 0.202 s. ± 0.020 s., from twenty-three stars with single values ranging from — 0.008 s. to — 0.413 s. In April of the next year, from the observation of twenty-four stars they found P—H = — 0.130 s. ± 0.008 s., with single values from — 0.068 s. to — 0.220 s.

The astronomers were unsatisfied with these results, and went on to the fixing of their absolute personal equations, using the transits of an artificial star, and taking the time with a Hipp chronoscope, measuring down to the thousandth of a second. Their artificial star, as seen in the telescope, was of the second or third magnitude. The disk (a hole in which made the star) was moved by a pendulum, and so fixed that through its eastward excursion it kept an electric circuit closed and through its westward left it open. In observing, an assistant let the pendulum fall, and this moved the disk from west to east. As the image of the star crossed the wire, the observer gave a signal to a second assistant, who instantly started the chronoscope. As the star crossed the wire on its return, it automatically broke the electric circuit, and thus threw the pointers of the chronoscope into connection with its driving machinery. As soon as the observer saw the star behind the wire, he pressed his key, thus closing the circuit again and throwing the pointers out of connection. The assistant at the chronograph stopped its works, read off the fraction of a second that had elapsed between the opening and closing of the circuit, and the apparatus was ready for another experiment.

The prime fault of the chronoscope for measurements of this kind is that it fails to record anticipatory estimates, and, unfortunately, Plantamour anticipated. To bring his anticipations into calculation, it was assumed that all the estimates would arrange themselves symmetrically about the mean of the figures that would remain after a number of the latest observations equal to that of the anticipations had been temporarily excluded. The difference between the mean of the figures retained and the mean of the figures excluded is by hypothesis equal to the difference between the first mean and the mean of the anticipations, and they can thus be brought into the general average. Hirsch always observed too late, and so his figures needed no correction. Plantamour's corrected averages are also too late.

The following are the values found:

For Plantamour:

		s. s.			
Nov. 4,	2d series,	0.103 ± 0.013	from	45	observations.
"	3d "	0.128 ± 0.014	"	33	"
"	5th "	0.048 ± 0.009	"	41	"
" 5,	1st "	0.069 ± 0.007	"	54	"
"	4th "	0.037 ± 0.006	"	37	"
	Mean,	0.060 ± 0.016			

For Hirsch:

		s. s.			
Nov. 4,	1st series,	0.247 ± 0.043	from	6	observations.
"	4th "	0.178 ± 0.014	"	19	"
"	6th "	0.140 ± 0.007	"	41	"
" 5,	2d "	0.199 ± 0.009	"	22	"
"	3d "	0.169 ± 0.008	"	23	"
	Mean,	0.168 ± 0.013			

From which P—H = — 0.108 s. ± 0.021 s. Or, taking the same by consecutive series:

Nov. 4,	series 2 and 1,	P—H	= — 0.144
"	" 3 " 4,	"	= — 0.050
"	" 5 " 6,	"	= — 0.092
" 5,	" 1 " 2,	"	= — 0.131
"	" 4 " 3,	"	= — 0.133
		Mean,	— 0.114 ± 0.019

The united results of all their comparisons, taken by days and weighted according to their probable errors, gave P—H = — 0.122 s. ± 0.026 s., or grouped by series, — 0.123 s. ± 0.015 s., and these values were finally taken as certain. The mean value of accidental variations in the same series was found equal to 0.056 s.; the value of changes due to the disposition of the observer ranged between 0.03 s. and 0.04 s. The changes from day to day seem about equal to those from year to year.

One of the most extended studies of the absolute personal equation was made in 1863 and 1864 by C. Wolf, of the Observatory of Paris. Like previous experimenters, he made use of artificial transits. The light of an oil lamp shining through a hole in a black screen made the star, which, by the interposition of lenses, appeared in the telescope as a very small point of light. The screen was moved evenly to and fro by a motor under control of the observer, and its rate could be changed by changing the driving weight. The reticle of the telescope used in the experiments was of five lines. The blunt end of a spring under the screen made contact with an adjustable strip of copper at every bisection of the star by a line of the reticle; or when the direction of its motion was reversed, broke contact at the same point. The making and breaking of the circuit caused the registry of the transit by an electro-magnet on a moving band of paper, where in like manner the clock-beats were also recorded. In some cases registry by an induction spark was substituted. The total length of the apparatus, as set up in Paris, was 4.25 meters. Forty transits, that is, the crossing of the reticle four times in each direction, constituted a full series. The offices of the different parts of the apparatus were so alternated during these as to exclude the errors of the instrument, and determinations by means of it are certain to 0.01 s.

Either the chronographic or the "eye and ear" method might have been studied, but Wolf considered that his personal equation by the chronographic method would be too small to afford hope of finding its laws, and therefore he confined his attention to the older method. In the first three months of experimentation his personal equation declined from 0.30 s. to 0.11 s., and afterward, under like circumstances, remained constant at that point, which he regards as an evidence of the value of training on appropriate apparatus. Since the observation of the star with motion both from right to left and *vice versa* was essential to the elimination of instrumental errors, Wolf found it necessary to investigate the influence of the direction of motion on the personal equation. The introduction of a total reflecting prism before the eye-piece each time the star moved in one direction or the other, and the removing it when the motion was reversed, made the star appear to move always in the same direction, and gave the means of studying the effect of direction without altering the action of the instrument. He found from twenty-two series of observations, with the star moving at the equatorial rate and the normal power of the eye-piece, the following means for his absolute error:

Motion from left to right,	0.14
" " right to left,	0.10
Difference,	0.04

That is to say, the distance of the star from the line whenever the star was found to the right of it at the beat of the second seemed relatively too large to him. Tests with dots and lines on paper gave a similar illusion and thus fixed its origin in the structure of the eye.

Wolf studied as well the effect of the degree and manner of the illumination of the field, the brightness of the star, and the position of the head of the observer, without finding that they influenced the size of the personal correction. The rate of the star made a difference. With variations which changed the time of crossing the five lines of the reticle from thirty-one to eighty-seven seconds, he found that his personal equation decreased with the rate, from 0.14 s. in the first case to 0.09 s. in the second ; the star at the equatorial rate crossed in sixty seconds and his personal equation was then 0.11 s. With increase in the power of the ocular his personal equation diminished, but this he does not regard as evidence of a law, because under the higher power a line of the reticle appears to have a sensible breadth, and the place of the star may not always be estimated from exactly the same point of it. And besides, the diameter of the image of the artificial star is increased, which is not the case with actual stars.

The second part of Wolf's study deals with the cause of personal equation, and as this is to be considered elsewhere, only his general conclusions will be given here. He would make three kinds of personal equation : the first, very rare, consists chiefly in a miscounting of the seconds, as is charged against Bessel ; second, a more common form and less in amóunt, caused by the difficulty of superposing the sensations, an error of imperfect training ; and third, the true physiological personal equation, much smaller and more constant, which is a function of the sensibility of the eye alone. The "eye and ear" method is necessarily better than the chronographic, because the latter involves the same question of the sensibility of the eye, and besides that, of the time necessary to press the key. If it be true, as Dunkin and Pape have shown, that the average error of a single observation is less by the chronographic method, it only argues that astronomers need more training.

Since the question with artificial-star measurements of the absolute personal equation is whether or not they really correspond to the conditions of actual observation, it is interesting to notice a suggestion made in 1867 by E. Kayser for the determination of the absolute personal equation from the transits of real stars. If a star is bisected by a wire of the reticle of an equatorial instrument, even if the driving clock have considerable errors, it will be able to preserve the bisection for a short time. It is only necessary, then, after making such a bisection, to stop the driving clock on a beat of the time clock and observe the transits of the star across the other wires, the true times of which can be accurately found from the known distances between them. Since the stopping of the clock on a second beat is only hitting one of a rhythmical series, the error need not be large. If the chronograph method is used, the thing is still simpler ; it is only necessary to make the stopping of the driving clock record itself on the chronograph. This gives a beginning from which the time of the transits, also recorded on the chronograph, can easily be read off and the absolute personal equations determined as before.

Since 1867 there have been a number of studies of personal equation, but most of them have been upon changes produced in its amount by varying conditions of observation. These it is proposed to bring in in their appropriate places (together with others passed by in the present narrative) in the next section, which is to deal with the variations of the personal equation.

MEMORY, HISTORICALLY AND EXPERIMENTALLY CONSIDERED.

W. H. BURNHAM, PH. D.

I.

AN HISTORICAL SKETCH OF THE OLDER CONCEPTIONS OF MEMORY.

I.—*Early Allusions to Memory.*

Mnemosyne, Hesiod tells us, was the mother of the Muses. Without speculating, as some have done, about the reasons for this myth, it is interesting as showing an appreciation of the fundamental nature of memory, and some sort of crude introspective psychology dating back possibly to prehistoric times. Before the art of writing was in common use, men had to depend more largely than to-day upon their memories. It is not surprising, then, that the ancients put a high estimate upon memory before they began to theorize about its nature. There are allusions to memory in Homer and in the Hebrew Scriptures,[1] and occasionally one of the early Greek philosophers tries to explain some phenomenon of memory, but we find no scientific study of the subject before Aristotle.

The psychology of the Ionian school of philosophers, as far as they had any, was sensationalism. Their views of memory must be conjectured from the fundamental principles of their philosophy.

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[1] For references see Carus: Geschichte der Psychologie, pp. 150 and 169.

The doctrine of transmigration, as held by the Pythagoreans, was in some degree an anticipation of Plato's doctrine of reminiscence; and Pythagoras's alleged belief that he remembered things that had happened in a former state of existence, would prove that he deemed memory an essential function of the soul, continuing beyond the limits of the present life.

Diogenes of Appolonia is said to have been puzzled by the phenomenon of forgetting. But, in accordance with the principles of his philosophy, he explained it by supposing that the cause of forgetting was an arrest of the equal distribution of air throughout the body. A corroboration of this explanation he found in the easier breathing that follows the recalling of what was forgotten.

Among the Eliatics, Parmenides is said to have held that not only thought, but recollecting and forgetting, depended upon the way the light or heat and the dark or cold are mixed in the body. If we may trust Theophrastus, every presentation, according to Parmenides, corresponded to a definite mixture relation of these qualities, and with the destruction of that relation the presentation disappeared, *i. e.*, was forgotten.[1]

Heracleitus, one might suppose, would study memory carefully; but, in the fragments of his philosophy that have come down to us, nothing is said upon the subject.

II.—*Plato's Doctrine of Memory.*

In Plato we find a more modern psychology. According to him, the thinking power of the mind, the understanding, is above the mere power of sense-perception. It is this power which compares and considers, notes similarities and contrasts, unity and plurality, and forms ideas of relation between Being and Non-being, as well as of relations of number and proportion. Among the elements of this higher power, recollection (ἀνάμνησις) is of prime importance. It rests upon association by similarity or contrast and by simultaneity.[1]

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[1] Cf. Siebeck: Geschichte der Psychologie, 1er Th., 1e Abth.

Plato distinguishes the passive retention of perceptions (μνήμη) from active memory (ἀνάμνησις),[2] and suggests as a definition of *memory*, "the power which the soul has of recovering, when by itself, some feeling which she experienced when in company with the body." He attempts no explanation of memory; but, in the Theaetetus, puts the following words into the mouth of Socrates:

"I would have you imagine, then, that there exists in the mind of man a block of wax, which is of different sizes in different men; harder, moister, and having more or less of purity in one than another, and in some of an intermediate quality. . . Let us say that this tablet is a gift of Memory, the mother of the Muses, and that when we wish to remember anything which we have seen, or heard, or thought in our own minds, we hold the wax to the perceptions and thoughts, and in that receive the impression of them as from the seal of a ring; and that we remember and know what is imprinted as long as the image lasts; but when the image is effaced or cannot be taken, then we forget and do not know."[3]

Plato carries out the same figure to explain different degrees of memory. When the wax in one's soul is deep, abundant, smooth, and of the right quality, the impressions are lasting. Such minds learn easily, retain easily, and are not liable to confusion. But, on the other hand, when the wax is very soft, one learns easily, but forgets as easily; if the wax is hard, the reverse is true; again, if the wax is hard or impure, the impressions are indistinct, and still more indistinct are they when jostled together in a little soul.

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[1] Phaedo, 73 and 74. [2] Phileb., 34. [3] Jowett's translation.

This illustration must not be taken too seriously, for later on in the same dialogue Socrates calls it a "waxen figment," and substitutes for it the figure of the aviary of all kinds of birds—"some flocking together apart from the rest, others in small groups, others solitary, flying anywhere and everywhere." This receptacle is empty when we are young. The birds are kinds of knowledge. Learning is the process of capturing the birds and of detaining them in this enclosure. In acts of memory we re-catch them and take them out of the aviary.

Plato's views upon memory have a special interest on account of their connection with his metaphysical doctrines. Perception and recollection are the occasion of the mind's turning away from the world of sense to the inner world of innate and universal ideas. These ideas we could never get from sense-perception. That gives us only the immediate and the individual. The ideas are of the essential and the universal. We could not conceive them if we did not already know them. Hence the power to know the universal in the individual proves a previous existence in which we had the intuitions of universal truths, and, accordingly, learning is but recollection.[1] The metaphysical aspects of memory, however, let us avoid as much as possible. They would soon lead far from a psychological study.

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[1] For references see Zeller's Plato and the Older Academy, pp. 126, 407; cf. also Siebeck: Geschichte der Psychologie.

But this doctrine of recollection lies at the heart of the Platonic philosophy, and it is necessary to note carefully the distinction between it and ordinary memory. The latter as defined by Plato in the passage quoted above, is the memory or recollection of what has been learned through the body, *i. e.*, through sense-perception. It belongs to the world of appearances and is liable to many errors. The former, on the contrary, is not concerned with things of sense. It is recollection of that higher world where we had an antenatal vision of intelligible realities. Its highest manifestation is the insight of the philosopher who sees the divine goodness, truth, and beauty.[1]

III.—*Aristotle on Memory.*

The difference of psychological method in Plato and Aristotle is seen in their treatment of the subject of memory. What Plato says of memory is incidental to the discussion of such profound matters as the nature of the soul and the theory of knowledge. Aristotle devotes a special tract to the subject of memory. While, according to Plato, memory is one of the higher faculties and partakes of the eternal nature of the soul, with Aristotle it is no longer a function of the eternal Nous, but has its seat in the *passive* reason, is dependent upon a physical process, and perishes with the body.

Aristotle seems to have been the first of ancient philosophers to write a systematic treatise on psychology. But, rather curiously, in this work on psychology there is no special treatment of memory. A special tract,[1] however, was devoted to the subject. So far as we know, this was the first scientific study of memory; and, for this reason, as well as for its intrinsic merits, the tract deserves special attention. But before passing to Aristotle's doctrine of memory, it is well to notice briefly his theory of sense-perception.

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[1] For the many passages in which the words *μνήμη*, *μνημονεύω*, *μνημονικός*, *υνήμων* occur in Plato, cf. Ast: Lexicon Platonicum, II., pp. 356, 357. For *ἀναμιμνήσκω* and *ἀνάμνησις* cf. the same, vol. I., pp. 151, 152.

On occasion of appropriate stimuli, movements take place in the sense-organs. These movements, however, are not sense-perception. In perception the mind must compare and distinguish disparate sensations; it must unite the sensations presented simultaneously by our double sense-organs, as of sight and hearing, and it must be conscious of sensation. This work of comparison, psychic synthesis, and self-conscious perception is performed by a central sense. The physical basis of this central sense is the heart. Through it the mind performs the act of sense-perception. Functions now attributed to nervous substance are referred by Aristotle to the *pneuma* connected with the blood. This is the medium by which the movements arising in the sense-organs are transmitted to the heart, and in this *pneuma* the movements persist after the external stimuli have ceased to act.

Incidentally it is interesting to note that, according to Aristotle's psychology, the brain has very little to do with mental activity; to borrow a phrase from Wallace, it serves simply as "a cooling apparatus to counteract the excessive warmth of the heart."

When the movement occasioned in the sense-organ by an external stimulus is propagated to the heart,[2] a perception occurs. Sense-perception, then, is an act of the soul by means of a physiological process. In the words of Aristotle, it is a "movement of the soul through the body."[1] This movement may continue after the stimulus which was the occasion of it has ceased to act. The extreme case is the well known phenomenon of a visual after-image. The images of the imagination are such after-sensations. Imagination is weak sensation, or, in the words of Hobbes, "decaying sense." So, too, dreaming is the result of a movement in our bodily organs, caused either from without or from within; and when the violent movements of the day are stilled, feebler movements that were ineffective during the waking hours may cause our dreams. Again, these persisting movements are the elements of memory. Memory and the imagination alike are dependent upon the residua of sensations. The subjective side of a sensation is an image. Thus the proper objects of memory, as well as of the imagination, are images (*φανταστὰ*). The image, according to Aristotle, is a condition (*πάθος*) of the central sense. Memory *per se* is of the original image or perception; only in an accidental manner does it relate to matters of thought. In other words, abstract ideas and the like are reproducible only so far as they imply images.

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[1] *De Memoria et Reminiscentia.* For a list of commentators, see Hamilton's edition of Reid's works, p. 891.

[2] The centre of touch and taste, according to Aristotle, is the heart. Sight, sound, and smell have their centre in the brain, but are indirectly connected with the heart.

At first one wonders how Aristotle will distinguish those movements which constitute memory from those which are the basis of imagination. He is not entirely satisfactory on this point, but he makes this distinction: the picture of the imagination, or the corresponding movement, does not refer to an external object, and is not located in the past. The memory picture, on the other hand, does refer to an object, and carries with it the consciousness of a time in the past when the perception remembered took place.[1] Memory, then, involves time, and both it and the sense of time are dependent upon the central sense.

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[1] De Somno, 1, 454: *ἡ δὲ λεγομένη αἴσθησις ὡς ἐνέργεια κίνησίς τις διὰ τοῦ σώματος τῆς ψυχῆς ἐστιν.*

In his special tract upon memory, Aristotle in part repeats Plato's views, in part discusses the obvious facts of memory which, having been continually repeated since his time, are now mere platitudes, and in part he tries to explain the phenomena of memory in accordance with his general system of psychology. The essay, however, is of special interest because in it Aristotle sets forth clearly the famous doctrine of the association of ideas. Some of the other points of the essay may be briefly mentioned, and special consideration given to the portion relating to association and recollection.

First, Aristotle takes a good deal of space to show, what would seem to be apparent enough to everybody, that memory is of the past, as perception is of the present, and hope and opinion of the future. He notes that the central sense, or sensorium, must be in a condition suitable to receive and retain the impressions. If the sensorium is too hard, no impression is made. If it is greatly agitated, the new movement is ineffectual —on somewhat the same principle, one may suppose, as we say in modern psychology that a weak stimulus is washed out by a strong one. Hence the very young and the very old have poor memories; for the former are in the movement of growth, the latter in that of decay. The question arises: How is it that in recollection we recognize the memory-image as a picture of the absent object? A scholastic answer is given.

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[1] See Wallace's Psychology of Aristotle, Introduction, pp. 93–94.

"An animal painted in a picture, he says, is both an animal and a copy, and while being that one and the same, it is nevertheless two things at once. The animal and the copy are not identical, and we may think of the picture either as animal or as a representation. This also is true of the image within us; and the idea which the mind contemplates is something in itself, although it is also the image of something else."[1]

The second chapter of the treatise on memory is devoted chiefly to recollection and the association of ideas. Aristotle distinguishes carefully the mere persistence and direct reproduction of a presentation (*μνήμη*) from voluntary recollection (*ἀνάμνησις*). The latter is indirect reproduction. It is possible only by the association of ideas. The former is an attribute of animals, while the latter is peculiar to man. Recollection occurs according to the sequence of ideas.[2] What and how necessary the sequence shall be depends upon our past experience. "If the sequence be necessary," Aristotle continues, "then when this movement occurs that one will follow. If it is not necessary, but a matter of habit, the latter movement will generally follow."

Sir Wm. Hamilton understands the word translated *movement* (*χίνησις*) as meaning merely *change in quality*. The word, then, he thinks may be fairly translated into modern nomenclature by his famous term *modification*.

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[1] Quoted from George Henry Lewes's Aristotle, p. 257.

[2] *συμβαίνουσι δ'αἱ αναμνήσεις, ἐπειδὴ πέφυκεν ἡ κίνησις ἥδε γενέσθαι μετὰ τήνδε.* This passage is obscure, but it is generally understood to refer to the sequence of motions or the corresponding ideas, and this interpretation agrees with the context. See Hamilton's edition of Reid, pp. 892, 893, and Themistius's Greek Paraphrase of De Memoria, quoted by Hamilton, pp. 893, 894; also Siebeck, Geschichte der Psychologie, Zweite Abtheilung, p. 77; Grote's Aristotle; Grant's Aristotle, p. 170.

One hesitates to criticise such a profound scholar and such a diligent student of Aristotle as Sir Wm. Hamilton; but in the light of what has been said, it seems much simpler, and more in accordance with the psychology of Aristotle, to understand his doctrine of recollection as follows: The *physiological* movements originally connected with a series of perceptions must occur again in the same order when we recall a true memory picture.[1] Man is so constituted that when one movement and the mental image connected with it occurs, another movement with its appropriate mental image is likely to follow. When we would recall anything, then, we must call up idea after idea until we arrive at one upon which in our experience the one we are in search of has often been sequent. Or, in terms of physiology, movement after movement must occur until we arrive at a movement upon which the movement corresponding to the idea desired has often been sequent.

This sequence or association of ideas is subject to certain laws. The remarkable passage in which Aristotle states these laws is translated by Sir Wm. Hamilton as follows: "When, therefore, we accomplish an act of reminiscence, we pass through a certain series of precursive movements, until we arrive at a movement on which the one we are in quest of is habitually consequent. Hence, too, it is that we hunt through the mental train, excogitating [what we seek] from [its *concomitant in*] THE PRESENT OR SOME OTHER TIME, and from its SIMILAR or CONTRARY or COADJACENT. Through this process reminiscence is effected. For the movements [which, and by which, we recollect] are, in these cases, sometimes the SAME, sometimes at the SAME TIME, sometimes PARTS OF THE SAME WHOLE."[1] Wallace, quoting the same passage in the introduction to his "Psychology of Aristotle,"[2] gives the following somewhat different and probably more accurate translation: "When engaged in recollection, we seek to excite some of our previous movements, until we come to that which the movement or impression of which we are in search was wont to follow. And hence we seek to reach this preceding impression by starting in our thought from an object present to us, or something else whether it be similar, contrary, or contiguous to that of which we are in search; recollection taking place in this manner because the movements are in one case identical, in another case coincident, and in the last case partly overlap."[3] Whichever translation we adopt, it seems plain enough that Aristotle maintained that voluntary recollection depends upon association by *similarity*, *contrast* or *contrariety*, and *contiguity*. Very likely he meant to include association by *simultaneity* and *sequence;* but any proof of this should rest upon the words used in the text and the general import of the passage, rather than upon doubtful emendations like some of Hamilton's.[4]

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[1] Cf. Siebeck: Geschichte der Psychologie, 1er Th., 2te Abth., p. 77 seq.

A more important question is whether Aristotle meant to limit the application of these laws to voluntary recollection (*ἀνάμνησις*), or whether he intended to include spontaneous reproduction (μνήμη) as well. The opinion commonly held by students of Aristotle, from Themistius down, seems to have been that he applied the law of association only to voluntary recollection. Hamilton, however, argues forcibly that Aristotle taught the universality of the law of association. It seems natural enough to suppose that one who saw so clearly that in the voluntary train of thought the sequence conforms to the law of association, would have seen that the same laws apply to the spontaneous activity of the mind. But, while Aristotle states the law of association clearly for the former, he at most merely alludes to the latter, and obscurely enough at that.

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[1] The Greek is, *διὸ καὶ τὸ ἐφεξῆς θηρεύομεν νοήσαντες ἀπὸ τοῦ νῦν, ἢ ἄλλου τινὸς καὶ ἀφ' ὁμοίου, ἢ ἐναντίου, ἢ τοῦ σύνεγγυς· διὰ τοῦτο γίνεται ἡ ἀνάμνησις. αἱ γὰρ κινήσεις τούτων τῶν μὲν αἱ αὐταί, τῶν δ' ἅμα, τῶν δὲ μέρος ἔχουσιν.*

[2] p. 95.

[3] See also Grote, Grant, Siebeck, and Zeller, op. cit.

[4] After *ἢ ἄλλου τινὸς* in the passage cited above, Hamilton would supply *χρόνου* or *καιροῦ*.

Later in the same treatise, Aristotle gives an illustration that may serve to elucidate the principles of association as he understood them. In recollection there are certain starting-points or clues. Milk suggests whiteness, whiteness the clear atmosphere, the atmosphere moisture, that the rainy season. So, too, Themistius, in commenting upon the passage quoted above, uses an illustration somewhat similar: "I see a painted lyre, and moved by this as the prior and leading image, I have the reminiscence of a *real lyre;* this suggests the *musician;* and the musician the *song* I heard him play."[1] Again, Aristotle uses an illustration somewhat as follows: Let A, B, C, D, E, F, G, H represent a series of ideas, one of which we wish to recall. From DE as a starting point, we may be moved forward by E, or backward by D, by the association of ideas. If, then, on the suggestion of DE we do not find what we would recall, we may find it by running over the series E...H; if not, we shall at any rate find the desired idea by running backwards from D to A. Not much stress, however, should be put upon this last illustration, for the text is so obscure that many different interpretations have been given by commentators. Perhaps Aristotle meant to illustrate something more profound than the mere linkings of presentations in a series and the process of recalling the mental train; but the illustration of such a simple matter as this was not unimportant in the first scientific study of memory. Such commonplace illustrations, however, would hardly be worth repeating, were it not that many have thought the doctrine of association a modern discovery. We have already seen that Plato refers to this doctrine. We shall soon see that St. Augustine held that without the presence of an associated idea we cannot recall a desired thought.

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[1] Quoted by Hamilton, Reid's Works, p. 901.

The place of memory in the Aristotelian psychology in relation to the lower psychic activities, is plain from what has been said. The relation of memory as voluntary recollection to the higher activity of the intellect is indicated by Aristotle when he says that recollection is a syllogistic process. Thus it is that while many animals have the lower kind of memory, man alone has the higher form. "The reason is," says Aristotle, "that reminiscence is, as it were, a kind of syllogism or mental discourse. For he who is reminiscent that he has formerly seen or heard or otherwise perceived anything, virtually performs an act of syllogism."[1] With Aristotle, the higher functions of the soul are based upon the lower. "Without nutrition there is no sense, without sense there is no phantasy, without phantasy there is no cogitation or intelligence."[1] The place of memory among the soul's functions is with the phantasy or imagination mediate between sensation and intelligence.

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[1] Hamilton's translation, Vol. II, p. 909, op. cit.

In connection with Aristotle's doctrine of recollection, one passage in his Psychology is interesting, although its importance has, perhaps, been exaggerated. "Recollection," he says, "starts from the soul, and terminates in the movements or impressions which are stored up in the organs of sense."[2] Siebeck interprets this passage as meaning that the soul has the power by means of the heart to effect a sort of efferent movement towards the sense organs, and thus to arouse anew the persisting residua of former motions. Recollection, then, with Aristotle, as in modern psychology, is an excitation, reproduced in a less degree, of the sense organs; and the same organs are excited and the same movements repeated as in the original sensation.[3] This passage is certainly a remarkable anticipation of Bain's famous doctrine that a reproduced impression "occupies the very same parts, and in the same manner" as the original impression.[4]

In the foregoing sketch of Aristotle's view of memory, the attempt has been made to give only what can fairly be found in Aristotle's text. Much of his tract upon memory is obscure. Commentators have held very conflicting opinions in regard to the importance of what he wrote upon association and recollection. Sir William Hamilton calls him the "founder and the finisher of the theory of association," looks upon the commentators as marvellously stupid in their interpretations, and deems it a proof of Aristotle's genius that it took the world 2000 years to become intelligent enough to understand him. In reading Hamilton's erudite discussion, one may be led almost to believe that Aristotle was the first Scottish philosopher. But, while Hamilton's Scottish apperception probably found too much in Aristotle's treatise, and while, on the other hand, Lewes may be right in saying that "here, as in so many other cases, modern knowledge supplies the telescope with its lenses," nevertheless Aristotle's doctrine of association was a valuable contribution to science, and it is manifestly unfair to charge him with ignorance of its importance because he did not spin out as many volumes upon the subject as the English Associationalists have done.[1]

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[1] Grote, op. cit., p. 211.

[2] Wallace: Aristotle's Psychology, p. 41.

[3] Siebeck, loc. cit., pp. 78–79.

[4] Bain: The Senses and the Intellect, p. 338.

IV.—*Conceptions of Memory among the Stoics and Epicureans, and in Cicero and Quintilian.*

The Stoics took Plato's figure of the wax almost literally. They held that the mind is originally a *tabula rasa.* Sensations are the first writing upon this tablet. The object of sensation makes an impression upon the perceiving subject as the seal impresses the wax. Memory depends upon this impression. This was the view of Zeno. Chrysippus found difficulties in such a crude materialistic theory. How could the mind receive and retain at the same time a number of different and partly incompatible impressions? Accordingly he replaced this view by the theory that the sense impression consists in a qualitative change (ἀλλοίωσις) of the passively receiving organ, the soul.[1] The presentation (φάντασμα) is a state of the soul. The relation of memory to the general theory of knowledge, with the Stoics, was briefly as follows: The lowest act of the soul is mere perception (αἴσθησις); the next is presentation (φαντασία), which adds conscious observation, its function being to make a first test of the truth of the material furnished by sense. If perception has offered a true picture of the external object, this presenting activity of the mind becomes so intensive that the understanding is brought into action. The understanding or judgment approves or disapproves the presentations. If it approves, then arises the empirical fact, which bears upon it the mark of truth. These facts memory stores up. By combination of the separate facts, empirical concepts are formed, which make up the treasure of memory or experience.[2]

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[1] For passages where the words μνήμη, ἀνάμνησις, etc., occur in Aristotle, see the index in the Berlin edition of Aristotle's Works, Vol. V. In addition to the works cited, see also Waddington-Kastus; De la psychologie d'Aristote, Chap. XIII.

The psychology of Epicurus and the other atomists was a simple kind of mechanical sensationalism. Eidola or images from external objects enter the soul through the sense organs. The mind stores up a great multitude of these eidola. Whenever we call up a picture of memory or the imagination, we turn the attention to one of these images. Thus the mind sees in the same way that the eye does, with this difference, that it perceives much thinner eidola.[3]

Cicero and Quintilian[4] both dwell upon the importance of memory, and both seem to adopt the common theory of the time, that impressions are stamped on the mind as the signets are marked on wax. They are especially concerned, however, with principles relating to the exercise of memory, and they give instructions for mnemonic aids in oratory. Cicero lays special stress upon order as an aid to memory; and, as sight is the most acute of the senses, those things are best remembered which are visualized by the imagination. In accordance with the ancient mnemonic systems, he would have these imagined forms localized. The advice of Quintilian in respect to memory is especially sensible. According to him, nothing can take the place of exercise and labor. Next in importance is the division and arrangement of one's subject. He notices also the importance of good health; and says that for slow minds, an interval of rest after study is a good thing to perfect the memory.

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[1] For references see Siebeck's *Geschichte der Psychologie*, p. 209. See also Ueberweg: History of Philosophy, Vol. I, p. 193.

[2] Cf. Stein: Die Erkenntnistheorie der Stoa; Zeller: Stoics, Epicureans, and Skeptics; and Ueberweg, loc. cit.

[3] Lucretius, IV, 750 seq.

[4] Cicero: De Oratore, II, 86 seq.; also Rhet. ad Herenn., III, 16–24; Quintilian: Instit., XI, 2.

V.—*Plotinus on Memory.*

The Neo-Platonic psychology of memory is represented by Plotinus.[1] He discusses the subject at considerable length and presents a somewhat original doctrine. Memory does not belong to God, nor to the divine immutable intelligence in man which knows by direct intellectual perception. It is a function of the soul, and first appears when the world-soul is individualized in bodies. Memory, however, has no basis in the physical organism, nor does the soul impress the sensations upon the body. The effects of sensations are not like impressions made by a seal, nor are they reactions (*ἀντερείσεις*) or configurations (*τυπώσεις*), but the mode of sense-perception is like that of intellectual activity. In memory, too, the soul is active, not passive. The influence of the body proves nothing against this. The changeable nature of the body may cause us to forget, but it cannot condition positive recollection. The body is the river of Lethe, but memory belongs to the soul. The part of the soul to which memory belongs is the image-forming faculty. This holds sense-impressions as well as thought. Two souls, the higher and the lower, are concerned in memory. When the soul leaves the body, the recollections of the lower soul are soon forgotten, in proportion as the higher soul rises toward the intelligible world.

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[1] Cf. Enn. IV, *L.* III, *C.* XXV-XXX, and *L.* VI.

VI.—*St. Augustine on Memory.*

St. Augustine developed the views of the Neo-Platonists in regard to memory. With him, memory is a faculty of animals, men and angels. God, whose immutable essence is above the sphere of movement and change, does not remember. Everything is seen by him in one indivisible and unchangeable present. Augustine does not agree with Aristotle that some animals are devoid of memory. He attributes memory even to fishes, and relates, in confirmation of this opinion, an incident that he had observed. There was a large fountain filled with fishes. People came daily to see them and often fed them. The fishes remembered what they received, and as soon as any one came to the fountain they crowded together, expecting their accustomed food. But Augustine does not suppose that animals have that higher memory which is purely intellectual, although he probably failed to see how purely mechanical and involuntary their so-called acts of memory are.

Memory, with St. Augustine, as in the psychology of Plotinus, is a function of the soul, not of the body. But, with Aristotle, he refers it to a seat in the physical organism.[1]

What is memory? It is thinking of what one knows. All the various modifications of the soul cannot all be present to us at once. There is a difference between knowing a thing and thinking of it. The musician, says Augustine, knows music, but he does not think of it when he is talking about geometry.[2] The ideas relating to music are in the mind in a latent state. Augustine anticipates Leibnitz in discussing the unconscious modifications of our ideas; but he speaks especially of their gradual decay, while Leibnitz considers the unconscious growth of them. "Many numbers,"[3] he says, "are gradually effaced from memory, for they remain not an instant unaltered. Indeed, what is not found in memory after a year is somewhat diminished even after one day. But this diminution is imperceptible; yet it is not wrongly inferred, for it does not suddenly all vanish the day before the year is up. Hence we may conclude that from the moment it was engraved in memory it began to slip away."[4]

The doctrine of unconscious mental changes and of unconscious mental states is one of the most remarkable features of Augustine's psychology. With irresistible logic he demonstrates the existence of such states in the following passage from another place: "But what when the memory itself loses anything, as falls out when we forget and seek that we may recollect? Where in the end do we search but in the memory itself? and there, if one thing be perchance offered instead of another, we reject it, until what we seek meets us; and when it doth, we say, 'This is it,' which we should not unless we recognized it, nor recognize it unless we remembered it. . . . For we do not believe it as something new, but, upon recollection, allow what was named to be right. But were it utterly blotted out of the mind, we should not remember it, even when reminded. For we have not as yet utterly forgotten that which we remember ourselves to have forgotten. What, then, we have utterly forgotten, though lost, we cannot even seek after."[1]

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[1] The seat of memory, with Augustine, however, is in the brain, not in the heart, as with Aristotle.

[2] De Trin., *L.* XIV, *C.* VII. See also Ferraz: Psych. de St. Augustin, 2d ed.

[3] Augustine does not mean to limit what follows to mathematical truths, but, according to his psychology, the same would be true of anything that we are liable to forget.

[4] De Musica, *L.* VI, *C.* IV.

It would not be difficult to find passages in modern psychologies that read almost like translations of this chapter of Augustine's Confessions.

Two kinds of memory—sense-memory and intellectual memory—are distinguished in the Augustinian psychology. The former preserves and reproduces not only the images of visible objects, but also the impressions of sounds, odors, and other objects which strike our senses.[2] The images are not like the *eidola* of Democritus, but are ideal, formed by the mind from its own essence. Intellectual memory contains our knowledge of the sciences, of literature and dialectic, and of the questions relating to these subjects.[3] This memory, unlike the memory of sense, contains not the images of things, but the things themselves. These ideas which the intellectual memory stores up are in a sense innate. They never came to us through the senses. They could never have been taught to us unless we had already had them in our memories. "When I learned them, I gave not credit to another man's mind, but recognized them in mine." Thus the memory contains the idea of truth and of God.

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[1] Conf., *L.* X, *C.* XIX. Pusey's translation.

[2] Conf., *L.* X, *C.* VIII.

[3] Conf., *L.* X, *C.* IX seq.

Augustine points out too, what has been repeated by Locke and others until it has become a platitude, that we do not remember objects themselves, but the ideas which we have obtained from them. And with his usual subtlety he shows that much of what is ordinarily attributed to perception is really the work of memory.

A French psychologist who has made a special study of St. Augustine, says: "We see what importance St. Augustine attaches to memory. It is, in his view, the faculty which preserves the ideas relating not only to the body but to the soul, not only to eternal truths but to the eternal Being himself. . . . This memory which is peculiar to man and which animals do not possess, this memory which in a mysterious manner contains in it intelligible realities, is, according to the Bishop of Hippo, one of the three great faculties of man and the origin of the other two. It is from it that intelligence arises, and the will proceeds from the one to the other and unites them. Thus, if it is allowed to compare things human with things divine, we have in us an image of the august Trinity. Memory, in which is the matter of knowledge, and which is as the place of intelligible things, offers some resemblance to the Father; the intellect, which is derived and formed from it, is not without analogy to the Son; and love or will, which unites the memory to the intellect, has a certain resemblance to the Holy Spirit."[1]

The well known conditions of a good memory, such as acuteness of sensation, order, and repetition, Augustine notices only incidentally. More attention is given to the relation of the will to memory and to the association of ideas.

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[1] Ferraz: Psych. de St. Augustin, p. 178; cf. also De Trin., *L.* XV, *C.* XXI, XXII, XXIII, and *L.* XI, *C.* VII and VIII.

Whether we remember or not depends upon the will. By an act of will we avert the memory from sense-perceptions; as, for example, when we hear a speaker and do not notice what he says, or read a page and do not know what we have read, or walk with our attention upon something else. In all these cases we perceive, but do not remember our perceptions. So, too, recollection depends upon the will: "As the will applies the sense to the body (*i. e.*, external object), so it applies the memory to the sense, and the eye of the mind of the thinker to the memory."[1]

This power of the will over memory is, however, limited by the association of ideas. In order to recall anything by a voluntary effort, we must remember the general notion of the thing or some associated idea. "For example, if I wish to remember what I supped on yesterday, either I have already remembered that I did sup, or if not yet this, at least I have remembered something about that time itself; if nothing else, at all events I have remembered yesterday, and that part of yesterday in which people usually sup, and what supping is."[2] In another place he says that of a series of ideas the lost part is recovered "by the part whereof we had hold."

Many since Augustine have marveled at the miracle of memory. None have expressed their admiration more eloquently. "Great is this force of memory," he exclaims, "excessive great, O my God! a large and boundless chamber: who ever sounded the bottom thereof? . . . A wonderful admiration surprises me, amazement seizes me upon this. And men go abroad to admire the heights of mountains, the mighty billows of the sea, the broad tides of rivers, the compass of the ocean and the circuits of the stars, and pass themselves by; nor wonder that when I spake of all these things I did not see them with mine eyes, yet could not have spoken of them unless I then actually saw the mountains, billows, rivers, stars which I had seen, and that ocean which I believe to be, inwardly in my memory, and that, with the vast spaces between, as if I saw them abroad. Yet did not I by seeing draw them into myself, when with mine eyes I beheld them; nor are they themselves with me, but their images only. And I know by what sense of the body each was impressed upon me."

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[1] De Trin., *L.* XI, *C.* VIII. Pusey's translation.

[2] De Trin., *L.* XI, *C.* VII. Pusey's translation.

It is an interesting fact that Augustine noticed the possibility of illusions of memory. Certain rare phenomena—the so-called recollections of Pythagoras and others who were said to have remembered objects perceived by the senses in a former state of existence—he explains in a very modern fashion, except that he attributes these beliefs to the agency of evil spirits. "For we must not," he says, "acquiesce in their story who assert that the Samian Pythagoras recollected some things of this kind which he had experienced when he was previously here in another body; and others tell yet of others that they experienced something of the same sort in their minds. But it may be conjectured that these were untrue recollections, such as we commonly experience in sleep, when we fancy we remember as though we had done or seen it, what we never did or saw at all; and that the minds of these persons, even though awake, were affected in this way at the suggestion of malignant and deceitful spirits, whose care it is to confirm or to sow some false belief concerning the changes of souls, in order to deceive men."[1] If they truly remembered such things, he argues, such phenomena would not be so rare, but many persons would experience the same.

Perhaps the most serious criticism of Augustine's psychology of memory is that he almost entirely neglects the physiological side of the subject. He does not even notice the relation of memory to states of health or disease, and of youth or age. In one place, however, he states that memory has its seat in one of the three ventricles of the brain which is situated between that which is the seat of sensation and that which presides over locomotion, so that our movements may be co-ordinated.[2] Certainly in some passages he seems to make memory contain a kind of innate ideas that may be drawn forth by suggestion.[3] But if here Augustine is unsatisfactory, it must be remembered that he is not writing a psychology, and that he was, as Ferraz calls him, a philosopher of transition. "He combats Plato's doctrine of reminiscence, and prepares the way for the innate ideas of Descartes, without positively enough rejecting the former and without clearly enough admitting the latter."[4]

The criticism has also been made that Augustine seems to waver in his conception of memory, that he sometimes represents it as the source of all our intellectual activity, comparing it among the other faculties to the Father in the Trinity, and that again he seems to limit this faculty to the work of preserving knowledge acquired empirically.

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[1] De Trin., *L.* XII, *C.* XV. Haddon's translation.

[2] De Gen. ad Litt., *L.* VII, *C.* XVIII.

[3] Conf., *L.* X, *C.* X and XI.

[4] Ferraz, op. cit., p. 192.

Without lingering upon the views of the other Church fathers,[1] Nemesius may be mentioned as an illustration of the continued Platonic influence. The soul is divided, according to him, into three parts, the perceiving (*φανταστικόν*), the thinking (*διανοητικόν*), and the remembering (*μνημονευτικόν*) faculties. The physiological basis of the third is the *pneuma* in the posterior ventricle of the brain. Besides the two aspects of memory usually mentioned, *i. e.*, retention and recollection, he treats reminiscence in the Platonic sense, *i. e.*, the becoming conscious of innate ideas.[2]

VII.—*Diseases of Memory mentioned by the Ancients.*

The pathological side of memory seems to have been little studied by the ancients. Augustine referred to the possibility of illusions of memory, in the passage already cited. Seneca tells of a certain Sabinus who had so bad a memory that he forgot the name of Ulysses, and again of Achilles, and sometimes of Priam, though he knew them as well as we remember our schoolmates.[3] Some remarkable cases of amnesia were reported by the elder Pliny. "Nothing whatever in man," he says, "is of so frail a nature as the memory; for it is affected by disease, by injuries, and even by fright, being sometimes partially lost, and at other times entirely so. A man who received a blow from a stone forgot the names of the letters only; while on the other hand, another person who fell from a very high roof could not so much as recollect his mother, or his relations and neighbors. Another person, in consequence of some disease, forgot his own servants even; and Messala Corvinus, the orator, lost all recollection of his own name." While these cases are good illustrations of certain diseases of memory, they are not reported with sufficient accuracy and detail to render them of much scientific value. Ancient thinkers appear not to have appreciated the importance of studying the pathological conditions of memory.

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[1] For an interesting criticism of Plato's doctrine of oblivience, see Tertullian, De An. Ch. XXIV.

[2] Cf. Siebeck, op. cit., 1er Th., 2te Abth., pp. 399, 400.

[3] Epistolae, 27.

VIII.—*Conceptions of Memory in the Middle Ages.*

The views of the scholastics need not detain us long. They seem to be generally developments, either of the views of Aristotle or of Plato and St. Augustine. Avicenna and others divided the inner or central sense of the Aristotelian psychology into five inner senses, of which one was memory. We have seen that, according to Aristotle's psychology, in perception the form or image of a thing enters the soul, being the subjective correlative of organic movements occasioned by external stimuli. In the scholastic psychology this view of perception becomes the doctrine of sensible species. The idea of memory held by many[1] was somewhat as follows: The impressions made by objects of sense are preserved by the mind. In recollection and imagination, the inner species corresponding to an external object can be formed by the aid of the physical organism without the actual presence of the object. The view of Albert the Great in regard to the localization of memory is interesting. The sensorium is in the anterior portion of the brain, judgment and the image-forming faculty farther back, and memory and recollection in the posterior portion.[1] The psychology of St. Thomas Aquinas, the famous pupil of Albert, is largely that of Aristotle. He speaks of the memory as the *vis apprehensiva prœterita*, and connects it with the sensory side of the mind, through the common or inner sense.

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[1] Cf. Siebeck, op. cit., 1er Th., 2te Abth., pp. 418, 433, for the views of Hugo of St. Victor and Avicenna.

Under the impulse of the Reformation, a new Aristotelianism arose. Its founder was Melancthon. He differs from Aristotle in making memory a function of the intellect, thus vindicating for it the immortality that Aristotle attributed to the active intellect.[2]

Ludovicus Vives, an Aristotelian of the sixteenth century, devoted considerable attention to memory and the association of ideas.[3] He wrote in a practical way about memory, and was, perhaps, the first to mention that mnemonic device which so many have found useful, *i. e.*, the writing of what one would keep in mind. It.is well, he says, to write what we would remember; the pen writes upon the heart as well as upon the paper. His doctrine of association is Aristotelian.

IX.—*Conceptions of Memory in Cartesian Philosophy.*

The notion of the correlation between physical and psychic processes. was clearly understood by the dualists of the seventeenth century. Chauvin's Thesaurus speaks of a threefold memory—that of the mind exclusively, that of the body, and that of mind and body. Of the last he says: "Memory of the mind and of the body consists in a constant relation between the thinking of the former and the motion of the latter, such that when a thought is recalled a movement is renewed; for the one state seems to call forth the other."[1]

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[1] Cf. Siebeck, loc. cit., p. 431.

[2] Cf. Ueberweg, Hist. of Phil., Vol. II, pp. 16–19.

[3] Cf. his Opera, passim, Basileae, 1555.

Descartes explains the physical processes involved in memory in accordance with his crude physiology, and bridges the chasm between mind and matter with dogmatism. "When the mind wills to recall anything," he says, "this volition causes the pineal gland to incline itself successively this way and that, and impel the animal spirits to various parts of the brain until they come to that part in which are traces left by the object we would remember." The nature of these traces is explained as follows: The pores of the brain, through which the animal spirits have once passed, acquire a tendency to open again in the same way to them as they come again, so that these spirits finding the same pores again enter them more easily than others. "In this way the spirits arouse a special movement in the gland, which represents the same object to the mind, and shows it that the object is the same which it wishes to recall."[2] In the passages quoted it will be observed that the mental process is not put as the result of the physical process, as in some later writers, but rather the action of the mind is emphasized as originating the physical process. Really, however, in the Cartesian philosophy neither process was looked upon as originating the other, but the constant correlation between the physical and the psychic was attributed to the ceaseless action of the Deity.

This idea of God's mediation between mind and matter was developed by Geulinx and Malebranche into the doctrine of occasional causes. On occasion of a physical process, God calls forth an idea in the mind. On occasion of a volition, God moves the body. This metaphysical doctrine must be borne in mind, especially in studying Malebranche's views of memory, or one may be tempted to see in them more than he meant to put there; for in some passages he writes in the style of modern psychology.[1] He treats the correlation (*liaison*) of ideas with traces in the brain and the correlation or association (*liaison*) of the traces with each other. The cause of the association of the traces in the brain, and of the corresponding ideas, is identity of time when the impressions were made. Traces impressed upon the brain simultaneously are revived together: for paths of association are opened between traces made at the same time, and the animal spirits can pass along these paths more easily than into other parts of the brain. Again, some of the traces are *naturally* associated one with another and with certain emotions, on account of an arrangement of the fibres that we have had from birth. Malebranche notices also the importance of the association of ideas in morals, politics, and all the sciences relating to man.

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[1] Chauvin: Thesaurus Philosophicus, Art. Memoria, Rotterdam, 1692.

[2] Descartes: De Passionibus, I, XLII.

Malebranche's view of the physiological processes connected with memory is similar to that of Descartes, and he deems the comprehension of the truth that all our varied perceptions are dependent upon cerebral changes sufficient for the explanation of memory. For "the fibres of the brain having once received certain impressions by the course of the animal spirits and by the action of objects, retain for some time a facility for receiving the same modifications. Now the memory consists merely in this facility, since one thinks of the same things when the brain receives the same impressions."[1] The similarity of memory and habit as far as physiological processes are concerned did not escape the notice of Malebranche. In a sense memory is a kind of habit, and apart from consciousness there would be no difference between it and the other habits.[2] Making due allowance for metaphysical interpretations and the Cartesian hypothesis of animal spirits, such teachings show that Malebranche was a pioneer in the field of physiological psychology.

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[1] Cf. Recherche de la Vérité, II, v, passim.

Spinoza's doctrine of memory is not very different from that of Malebranche, in spite of the difference in their philosophical systems. He is brief upon the subject, but explains the way the thought of one thing suggests that of another, and gives the essentials of the modern doctrine of the association of ideas. Memory depends upon this association of ideas; "for it is nothing else than a concatenation of ideas implying the nature of things outside the human body, which is formed in the mind according to an order and concatenation of physical states."[3]

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[1] Op. cit., II, v, 3.

[2] Op. cit., II, v, 4: Il est visible, par ce que l'on vient de dire, qu'il y a beaucoup de rapport entre la *mémoire* et les *habitudes*, et qu'en un sens la mémoire peut passer pour une espèce d'habitude. Car, de même que les habitudes corporelles consistent dans la facilité que les esprits ont acquise de passer par certains endroits de notre corps, ainsi la mémoire consiste dans les traces que les mémes esprits ont imprimées dans le cerveau, lesquelles sont cause de la facilité que nous avons de nous souvenir des choses. De sorte que s'il n'y avait point de perceptions attachées aux cours des esprits animaux, ni à ces traces, il n'y aurait aucune différence entre la mémoire et les autres habitudes.

[3] Ethices, Pars II, Prop. XVIII.

X.—*Views of Early English Writers and of Leibnitz.*

Bacon writes sensibly of memory, giving special attention to the "Helps of memory."[1] He speaks slightingly of the mnemonic art as practiced in his day, esteeming the mnemonic feats of its devotees as no better than the "tricks and antics of clowns and rope-dancers." He was convinced that there might be better precepts and a better practice of the art than those in vogue. The art of memory, according to Bacon, is built upon what he calls Pre-notion and Emblem. Order, artificial places, and verse aid memory by giving a pre-notion of what the thing is we would recall. If we try to recollect a thing and have no pre-notion of what we would recall, "we seek and toil and wander here and there, as if in infinite space." But a pre-notion cuts off infinity and limits the range of memory. Emblem reduces intellectual conceptions to sense-images. We can remember an object of sense more easily than an object of the intellect. Hence the advantage of associating what is to be remembered with an emblem.

Memory has an important place among the faculties. In his psychology the three great powers of the human mind are Memory, Fancy, and Reason. Corresponding to this division of the faculties, he makes his famous threefold division of the sciences into History, Poetry, and Philosophy.

There is little new in the psychology of memory taught by Hobbes.[2] It is essentially that of Aristotle. All our knowledge originates in sense. The cause of sensation is physical motion. Memory and imagination are "decaying sense." "This *decaying sense,* when we would express the thing itself, I mean *fancy* itself, we call *imagination* . . . but when we would express the decay, and signify that the sense is fading, old, and past, it is called *memory*." Thus memory and imagination are one thing with different names. Again, he terms remembrance a sixth sense, because it is concerned with the past. Names according to him are chiefly mnemonic devices. And he writes in Aristotelian fashion upon the association of ideas.[1]

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[1] Cf. especially De Aug., V, 5; Nov. Org., II, 26.

[2] Cf. Human Nature, passim.

The next English philosopher to be considered is Locke. He uses metaphorical language in discussing memory, speaks of the "repository of the memory" and the "constant decay of all our ideas, even of those which are struck deepest," says that when the ideas are not renewed "the print wears out," that "the pictures drawn in our minds are laid in fading colors," that "our minds represent to us those tombs to which we are approaching, where though the brass and marble remain, yet the inscriptions are effaced by time and the imagery moulders away," that in recollection our ideas are often "roused and tumbled out of their dark cells into open daylight by turbulent and tempestuous passions." But these are merely figures of speech. The psychology of Locke is very different from that of the Cartesians or that of Leibnitz. Not only has the mind no innate ideas, but it has no unconscious ideas. The pith of his doctrine of memory is expressed in the following passage: "But our ideas being nothing but actual perceptions in the mind, which cease to be anything when there is no perception of them, this laying up of our ideas in the repository of the memory signifies no more but this, that the mind has a power in many cases to revive perceptions which it has once had, with this additional perception annexed to them, that it has had them before. And in this sense it is that our ideas are said to be in our memories, when indeed they are actually nowhere, but only there is an ability in the mind when it will to revive them again, and as it were paint them anew on itself, though some with more, some with less difficulty ; some more lively, and others more obscurely." Locke distinguishes in the general faculty of *retention* two kinds of activity : First, the keeping of ideas for some time before the mind. This is *contemplation*. Second, the reviving of ideas without the help of the objects which first caused them. This is *memory*. Attention, repetition, pleasure and pain are the means of fixing ideas.

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[1] Human Nature, IV, 2; also Leviathan, Ch. III.

Locke does not discuss the physiological aspects of memory, but, from pathological phenomena, concludes that the constitution of the body may influence the memory. And from observation of animals, especially of birds learning tunes, he infers that several other animals as well as man have the faculty of retention. His chapter upon the association of ideas is a suggestive discussion of the influence of the habitual union of ideas upon the opinions, reasonings, and actions of men.[1]

Leibnitz not only opposed the empiricism of Locke and argued for innate ideas, or at least natural dispositions and tendencies, but he combated the notion that by memory we mean merely the faculty of recalling ideas at pleasure, the ideas having no existence when not before the mind. "If nothing remained," he says, "of past thoughts as soon as we cease to think of them, it would not be possible to explain how one retains the memory of them; to have recourse to that bare faculty for explanation, is to say nothing intelligible." Leibnitz favored rather the Cartesian hypothesis, that the conditions of memory are traces or dispositions left in the soul as well as in the body by former impressions. These traces remain, though we are unconscious of them; and also the effects of things we cannot recall may remain in the mind.[1]

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[1] Essay concerning Human Understanding, Book II, Chaps. X, XIX, XXXIII.

Wolff, the disciple of Leibnitz, makes memory a faculty of the soul, and the Leibnitzo-Wolffian psychology prevailed in Germany until Kant.

XI.—*Conceptions of Memory in the 18th Century.*

The historical study of memory has more than a psychological interest. The subject is connected with some of the profoundest questions of philosophy. About it materialists against idealists, and empiricists against nativists, have fought their battles. The doctrine of the Cartesians, that memory was conditioned by traces left in the brain, was developed by the physiologists and Encyklopedists of the 18th century into a materialistic and mechanical view of memory. Sometimes the help of mathematics was sought to make the mechanical view more definite. The physiologist Haller performing the first experiments upon the time occupied in psychic processes, had estimated that a third of a second was sufficient time for the production of one idea. On this basis Hook and others reckoned that in a hundred years a man must collect 9,467,280,000 traces or impressions of ideas in his brain. Reducing this to a third on account of sleep, etc., one has 3,155,760,000, or in fifty years 1,577,880,000 traces. Assuming the weight of the brain to be four pounds, and deducting one pound for the weight of the vessels and blood, and another for the weight of the cortex (which, strangely enough, they thought did not have the power of preserving impressions), 205,542 traces must be found in one gram of the nerve-substance of the brain.[1] Haller, however, while basing memory upon traces in the brain, admitted his ignorance of the nature of these traces, and opposed materialism.

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[1] Nouveaux Essais, I, iii. 18, and II, x. 2.

Condillac is, perhaps, the best representative of the psychology of the Encyklopedists. In his famous statue gradually endowed with sense he traces the development of memory and imagination.[2] When the statue is endowed with the sense of smell, he says that this supposed man would have no knowledge of the relation of things without memory. He would suffer and rejoice without having either desire or fear. "But the odor which the statue perceives does not entirely escape from it as soon as the odorous body ceases to act upon its sense-organ. The attention bestowed upon the sensation retains it still; and there remains an impression, more or less strong according as the attention itself has been more or less active. This is memory."[3] When the statue perceives a new odor, that which it had a moment before is still present. The power of perceiving is divided between the remembered sensation and the present odor. The statue compares the two and learns to judge. By repeated exercise, acts of memory, comparison, and judgment become habitual. At the first sensation experienced, the statue has no surprise, for it has been accustomed to no other sensation; but when it passes from an accustomed to a totally different state, then it experiences surprise. Surprise arouses the attention. If the odors equally attract the attention, they are stored in memory in the order of their succession. If there are a great many impressions in the succession, then the last and the most novel will be the strongest. In memory, then, we have a series of ideas which form a kind of chain. This linking of the ideas furnishes the means of passing from one idea to another and of recalling the most distant. The great law of association of ideas is coexistence in time. Two degrees in the power of recollection may be distinguished: one weak, where a thing is recalled as past; the other strong, where a thing is recalled so vividly that it seems to be present. The one is called memory, the other imagination. These two faculties differ only in degree. Memory is weak imagination. Imagination is the most vivid kind of memory.[1]

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[1] Cf. Huber: Das Gedächtniss, p. 21.

[2] Traité des Sensations, passim.

[3] Op. cit., Part I, Ch. II, §6.

The great mystery of memory, Condillac treats as follows. Where, he asks, is the idea of a thing when for a long time the mind does not think of it? It is not in the mind; for disease can destroy the power to recall it. It is not in the body. Only a physical cause could preserve it there; and it would be necessary to suppose that the brain remains in exactly the same state in which it was put by the sensation which the statue would recall. Moreover, it would be difficult to reconcile this hypothesis with the continual movements of the animal spirits, and with the multitude of ideas with which memory is enriched. Condillac gives what he deems a simpler explanation. "I have a sensation," he says, "when a movement occurs in one of my organs and is transmitted to the brain. If the same movement begins in the brain and is propagated to the sense-organ, I believe that I have a sensation that I do not have: it is an illusion. But if this movement begins and ends in the brain, I remember the sensation that I have had. When an idea returns to the statue, it is not that it has been preserved in the body or in the mind: it is only because the movement which is the physical and occasional cause of it, is reproduced in the brain."[1]

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[1] Op. cit., I, ii. 29.

Although, for the sake of simplicity, Condillac first endows his supposed statue with the sense of smell, and traces the development of memory while it has this one sense, he looked upon the sense of touch as the basis of all the ideas we retain in memory, and thought the memory of the ideas which arise from touch stronger and more enduring than that of ideas coming from the other senses.[2]

Helvetius considers a few special points.[3] He attempts to show that a great intellect does not necessarily imply a great memory, and concludes that, on the contrary, extreme capacity of the one is exclusive of great capacity of the other. The relation of memory to the intellect is expressed in the following sentence: "Memory is the storehouse where are deposited sensations, facts, and ideas, the various combinations of which form what we call intelligence (*esprit*)."

Helvetius considers that memory is almost entirely factitious. The great differences in memory among educated men are due less to differences of natural endowments than to different degrees of training. Men with feeble memories, like St. Augustine and

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[1] Op. cit., I, ii. 38.

[2] Op. cit., Partie II, Ch. XI.

[3] Discours de l'esprit; disc. III, c. 3.

Montaigne, learned much on account of their great desire to learn. The capacity of memory depends upon three things—on the daily use of it, on attention, on the order in which one arranges one's ideas. He emphasizes the last. A great memory is, as it were, a phenomenon of order.

Helvetius further attempts to prove that all men are endowed with sufficiently good memories to enable them to attain a high degree of intellectual culture. "Every man," he argues, "is really sufficiently favored by nature in this respect, if the storehouse of his memory is capable of containing such a number of ideas or facts that by constantly comparing them he can always perceive some new relation, always increase the number of his ideas, and consequently always increase the capacity of his intellect. Now, if thirty or forty objects, as geometry shows, can be compared in so many ways that, in the course of a long life, no one can observe all their relations nor deduce all the possible ideas from them; and if among men whom I call well endowed (*bien organisés*), there is no one whose memory cannot contain, not only all the words of a language, but also an infinity of dates, facts, names, places, and persons, and, finally, a number of objects considerably more than six or seven thousand, I conclude confidently that every well-endowed man is given a capacity of memory far beyond what he can make use of for increasing his ideas; that greater capacity of memory would not give greater capacity of intellect; and thus that, instead of regarding the inequality of memory in men as the cause of the inequality of their intellects, this latter inequality is entirely the result, either of the attention, greater or less, with which they observe the relations of objects, or of the bad choice of objects with which they load their memories. There are, indeed, barren objects, and those, such as dates, names of places, persons, and other like things, which occupy a large place in the memory without being able to produce either a new idea or one interesting to the world . . . . This is why one is seldom a great man who has not the courage to be ignorant of an infinite number of useless things.'"[1]

One of the greatest physiologists and thinkers of the eighteenth century was Bonnet. His views of memory are much like those of Condillac.[2] Yet he opposes materialism, and claims that man is no more all matter than all spirit; he is rather an *Etre-mixte*. Bonnet's method was, he says, to look for the immediate antecedents of a thing. Before searching for the way an idea was reproduced, he inquired how it was produced. All our ideas, according to Bonnet, are derived from sense. The kind of sensation depends upon the anatomical structure of the sense-organ. Not only do the different sense-organs have different nerve-structure, but the fibres of the same sense-organ vary in structure.[3] The phenomena of refrangibility of the rays of light and the vibration of the cords of sonorous instruments strengthened this conjecture. "Each perception," he says, "has its character which distinguishes it from every other. For example, each ray of color has its essence, which is invariable. A red ray does not have precisely the same effect as a blue ray. There are then also, among the sight fibres, differences corresponding to the differences in the rays."[1]

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[1] Discours de l'esprit; disc. III, c. 3.

[2] Cf. Contemplation de la Nature, Part V, Ch. 6; Essai de Psych., Ch. 4, 5, 6, 27, 31; Essai analytique sur l'âme, Ch. 7–9; Analyse abrégée de essai analytique, ¶ 7–11; Essai d'application des Principes Psych. de l'Auteur, passim; Philalethe, Ch. 3; Palingénésie, Part II, Ch. 1.

[3] Analyse abrégée, ¶ IX: "Si chaque sens a sa mechanique, j'ai cru que chaque espèce de fibre sensible pourrait avoir la sienne."

The physical correlative of a sensation is a movement or vibration in the fibres of the sense-organ. The reproduction of a sensation likewise depends upon a physiological process. Memory and imagination have their seat in the body. This is proved by pathological cases in which accidents affecting only the body weaken and destroy these powers. Anticipating Bain, he says that the reproduced impression depends upon the vibration of the same nerve-fibres as transmitted the original impression to the mind.[2] The fibres that transmit and reproduce impressions have a structure adapted to this double function. "The sense-fibres are constructed in such a way that action more or less prolonged of objects produces in the fibres determinations more or less durable." Admitting his ignorance of the structure of the sense-fibres, Bonnet did not venture to explain the nature of these determinations.

His general doctrine of the physics of memory is well expressed in the following passage: "Not only does the fibre transmit to the mind the impression of the object; but it also retraces the memory of that impression. This memory differs from the sensation itself only in the degree of intensity. It has then the same origin; it then, as well as the sensation, depends upon a movement excited in the fibre, but a feebler movement. The execution of this movement demands a certain disposition in the constituent parts of the fibre. The elements retain then, for a longer or shorter time, the determinations which they have received from the action of the object. It strings the fibre, so to speak, to its tone; and, as long as the fibre remains thus strung, it preserves a tendency to retrace to the mind the memory of the sensation from the object."[1] From the passage just quoted we see that, when a vibration has once occurred, a certain flexibility imparted to the nerve makes it easier for the same vibration to occur again than for a new movement to take place. This increased facility of vibration is the cause of our recognizing a sensation or an idea when it occurs a second time.[2]

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[1] The doctrine of specific nerve energy here expressed is noteworthy. Cf. also Contemplation de la Nature, Tome I, pp. 97 and 98, 2d ed., Amsterdam. "Chaque sens renferme donc probablement des fibres *spécifiquement* différentes. Ce sont autant de petits sens particuliers, qui ont leur manière propre d'agir, et dont la fin est d'exciter dans l'âme des perceptions correspondantes à leur jeu."

[2] Analyse abrégée, ¶ IX: "C'est à l'ébranlement de certaine fibres, que cette sensation a été originairement attachée. Sa *réproduction* ou son rappel par l'imagination, tiendra donc encore à l'ébranlement de ces mêmes fibres."

The connection between habit and memory, which Malebranche had so well pointed out, did not escape the observation of Bonnet. He saw, too, that the nerves depend upon nutrition, and that they retain in growth their functional dispositions. To quote his words: "The sense-fibres depend upon nutrition like all the other parts of the body; they assimilate or incorporate alimentary substances; they grow; and, while receiving nutriment and growing, they perform their peculiar functions, and remain in essence unchanged. Their mechanism is, then, such that they incorporate nutriment in direct conformity with their structure and their acquired tendencies. Thus nutrition tends to preserve in the fibres these tendencies and cause them to take root; for they increase in stability in proportion as the fibres grow, and, I believe, we see here the origin of habit, that powerful queen of the sentient and intelligent world."[1]

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[1] Analyse abrégée, ¶ X.

[2] Cf. Essai de Pysch., Ch. V; and Essai an. sur l'âme, Ch. IX.

The views of Bonnet, probably more than of any other philosopher we have studied, have the ring of modern physiological psychology. He announces clearly that psychic processes have their correlative in physical processes. The brain is the organ of mind. And the tenacity of memory depends upon the ability of the brain elements to retain determinations imparted to them. As the tendencies to particular modes of vibration preserved in the brain were caused by the action of external objects, it is, in a sense, a mirror of a portion, larger or smaller, of the universe. In writing of this Bonnet grows eloquent. "What images," he exclaims, "are those in the brain of a Homer, a Virgil, or a Milton! What mechanism executes those marvellous scenes! The intelligence which could have read in the brain of Homer, would have seen the Iliad represented by the varied play of a million fibres."[2]

It should be noticed especially that Bonnet was not a materialist. He was, however, ready to accept any results that investigation might furnish. His fearless attitude toward materialism may be inferred from the following passage: "If some one should ever demonstrate that the soul is material, far from being alarmed at it, one should wonder at the power which had given matter the ability to think."[3]

Among English philosophers of the eighteenth century, Hume has something to say about memory and the association of ideas.[4] He reverses the distinction that Condillac makes between memory and imagination. The difference lies in the "superior force and vivacity" of memory. "A man may indulge his fancy in feigning any past scene of adventures; nor would there be any possibility of distinguishing this from a remembrance of a like kind, were not the ideas of the imagination fainter and more obscure."[1] The ideas of memory often degenerate and we are unable to distinguish them from the ideas of fancy. On the other hand, illusions of memory are possible. "An idea of the imagination may acquire such a force and vivacity as to pass for an idea of the memory, and counterfeit its effects on the belief and judgment. This is noted in the case of liars, who, by the frequent repetition of their lies, come at last to believe and remember them as realities; custom and habit having in this case, as in many others, the same influence on the mind as nature, and in fixing the idea with equal force and vigor."[2]

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[1] Contemplation de la Nature, Tome I, pp. 99 and 100, 2d ed.

[2] Ibid.

[3] Analyse abrégée, ¶ XIX.

[4] Cf. especially Treatise of Human Nature, I, iii. 5; I, i. 3 and 4.

The laws of association according to which simple ideas are united into complex ones are three in Hume's psychology—*resemblance*, *contiguity* in time or place, and *cause* and *effect*. The last is most extensive. Even here, however, there is no necessary connection; but the idea of causality is the result of an experience of uniform sequence. Of association he says in a famous passage: "Here is a kind of *attraction*, which in the mental world will be found to have as extraordinary effects as in the natural, and to show itself in as many and various forms."[3]

Hume discusses the relation of memory to the problem of personal identity. Memory is the chief source of personal identity, because it makes known to us the extent and continuity of the succession of our perceptions. But it does not produce, it rather *discovers* to us personal identity, by showing us the relation of cause and effect among our perceptions. "Who can tell me," asks Hume, "what were his thoughts and actions on the first day of January, 1715?" Nevertheless, though one cannot recall what he did on a given date several years in the past, yet no one doubts the identity of that past self with his present self. Thus Hume argues: "It will be incumbent on those who affirm that our memory produces entirely our personal identity, to give a reason why we can thus extend our identity beyond our memory."[1]

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[1] Op. cit., I, iii. 5.

[2] Ibid.

[3] Op. cit., I, i. 3.

Hartley, the eminent English physiologist, in accordance with the general principles of his psychology, outlines an interesting form of the vibratory theory of memory[2] not essentially different from that of Bonnet. Influenced by Newton, he believed that ether pervades all things, even the most solid bodies. In sense-perception, vibrations imparted by the ether are transmitted by the nerves to the brain and there transmuted into sensations. The sensation is the subjective aspect of the vibration. When the sensory vibrations cease, dispositions to diminutive vibrations persist in the medullary substance of the brain. These diminutive vibrations, the physical correlatives of decaying sense, Hartley calls vibratiuncules. They are the condition of our ideas. The vibratiuncules from sensations that were simultaneous or successive become associated, so that we have clusters of impressions and complex ideas. In memory the vibratiuncules are renewed, and the same ideas and clusters of ideas are revived.

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[1] Op. cit., I, iv. 6.

[2] Cf. Observations on Man, Sect. IV., Prop. XC.

Memory depends entirely upon the state of the brain. For it is impaired or destroyed by diseases, concussions of the brain, spirituous liquors, and the like, and generally returns with the return of health. If sensations and ideas arise from vibrations and dispositions to vibrate in the medullary substance of the brain, it is easy to see that these causes would disturb the order of ideas.

From the subjective standpoint Hartley defines memory as "that faculty by which traces of sensations and ideas recur or are recalled in the same order and proportion, accurately or nearly, as they were once actually presented."[1] Thus memory is based upon the association of ideas. The great law of association is that of contiguity in time or space. Hartley considered at great length the association of the vibratiuncules, and of the corresponding ideas, attempting to show that all reasoning and affection are the result of association. He at least succeeded in laying the foundation of the modern Associational Psychology.[2]

Hartley considers the various phenomena of memory, such as the defects of memory in children, old people, and diseased persons, and tries to make them tally with his theory. According to his psychology, memory is a fundamental power of the mind. All our voluntary powers are of the nature of memory. The results of observation in pathological cases agree with this; for in diseases of memory the voluntary actions are imperfect. Taking memory in a large sense, all the powers of the soul may be referred to it. Thus strong power of retention is indispensable to strong judgment; and though some persons with weak judgments may have strong memories, no one with a weak memory can have a strong judgment.

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[1] Op. cit., Introduction, p. 2.

[2] For a good history of the doctrine of association of ideas see Ferri: La Psych. de l'Association depuis Hobbes jusqu'à nos jours; also for older views, Hissmann: Geschichte der Lehre von der Association der Ideen; Göttingen, 1777.

It is interesting to note, in connection with such studies as those recently made by Kraepelin,[1] that Hartley, as well as Hume and others, noticed the possibility of illusions of memory. The difference between memory and reverie consists, he thinks, in the greater vividness of the clusters of memory pictures, and principally in the readiness and strength of the associations by which they are united. Many persons, he points out, are known by some false story that they relate over and over. By magnifying the ideas and associations they at last come to believe that they remember what they tell. The story makes as vivid an impression on them and hangs as closely together as any assemblage of past facts in their memory. Thus "all men are sometimes at a loss to know whether clusters of ideas that strike the fancy strongly, and succeed each other readily and immediately, be recollections or mere reveries. And the more they agitate the matter in the mind, the more does the reverie appear like a recollection." As when in endeavoring to recollect a verse, a wrong word suiting the place, and afterward the right one occurs, one sometimes becomes confused, and for the moment it is hard to distinguish the right one. "Persons of irritable, nervous systems are more subject to such fallacies than others. And madmen often impose on themselves in this way, viz., from the vividness of their ideas and associations, produced by bodily causes. The same thing often happens in dreams. The vividness of the new scene often makes it appear like one that we remember and are well acquainted with."

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[1] Archiv f. Psychiatrie, 1886 and 1887.

XII.—*Mnemonic Systems.*

No historical sketch of the doctrines of memory among the older writers would be complete without some mention of their mnemonic systems. The art of mnemonics seems to have been much in vogue among the ancient Greeks and Romans. Every scholar of the classics is familiar with the story that ascribes the invention of the art to Simonides. There are allusions to this art in the works of Aristotle, Plato, and other classic writers. Aristotle is reported by some to have written a work upon mnemonics. Cicero and Quintilian give special attention to the subject.[1]

The main principles of the ancient mnemonic systems were somewhat as follows: The thing to be remembered was localized by the imagination in some definite place—say in a room of a real or imaginary house; and, if necessary, a concrete symbol as vivid as possible was associated with it. Often a large house was visualized in the imagination, and the rooms, walls, furniture, statues, etc., associated with things to be remembered. To recall anything it was only necessary to rummage about in this imaginary house until one found what was desired. This device was much used among the Romans as an aid to oratory; and it has been said that the phrases, *in the first place, in the second place,* and the like originated in this ancient practice.

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[1] Cicero: De Oratore, II, 86–88; Rhet. ad Herenn., III, 16–24. Quintilian: Inst. XI, 2. Cf. also Pliny: Hist. Nat., VII, 24.

From the fifth to the thirteenth century the mnemonic art may have been practiced in the monasteries, but we hear little of it. In the thirteenth and fourteenth centuries the ancient systems were revived. Roger Bacon was one of the writers upon the subject; but his work was never printed, though it is said to be still preserved in MS. at Oxford. Toward the close of the fifteenth century the famous teacher, Petrus de Ravenna, appeared, and the first edition of his *Ars Memorativa* was published in 1491. In the sixteenth and seventeenth centuries a great many books upon mnemonics were published. Among the most important were the works of Lamprecht Schenkel and Giordano Bruno. Winkelmann and Leibnitz invented, or borrowed from the Hebrew Bible, the device of representing figures by letters. And later Grey made special application of this principle in his *Memoria Technica* which appeared in 1730.

The character of some of the mnemonic teachers of this period may be inferred from the following passage from Cornelius Agrippa's *De Vanitate Scientiarum.* Speaking of the vanity of the mnemonic art where there is not a good natural memory to begin with, and of the authors who have written upon the subject, he says: "Many there be that at this day profess the same, though they get more infamy and disrepute than gain thereby; being a sort of rascally fellows that do many times impose upon silly youth, only to draw some small piece of money from them for present subsistence."[1] There is at least this difference between the mnemonic teachers of Agrippa's time and those of the present. The latter generally get, not a small piece of money, but a large piece, and they sometimes impose upon others as well as silly youth.

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[1] The Vanity of Arts and Sciences; Eng. translation: London, 1676. For a modern example of the mnemonic money-getter, see "*Loisette*" *Exposed*, by G. S. Fellows; New York, 1888.

Most of the systems taught before the time of Grey seem to have differed little from the ancient systems. Localization and visualization were the characteristics of them. Sometimes mnemonic towns, with numerous streets, squares, and buildings, were formed. By continued thought the mnemonic expert became at home in this imaginary town. It was laid out probably according to the classification of the sciences, and by this device the abstract was associated with the concrete, and the imagination brought to the aid of memory.

The ancient and mediæval systems of mnemonics are inferior to the best modern systems, especially that of Pick,[1] which is based upon sound psychological principles. But they were probably very helpful to eye-minded people. The men with remarkable memories mentioned by Cicero and others probably owed much to mnemonic aids. It is of special psychological interest to consider the ancient mnemonic devices in connection with such studies as those of Galton upon mental imagery, number forms, and the like.[2] The prevalence of these systems may indicate that the faculty of visualization was highly developed in many of the ancient Greeks and Romans, and among the devotees of their mnemonic systems in the Middle Ages.[3]

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[1] Pick: Memory and the Rational Means of Improving it. London, 1861.

[2] Enquiry into Human Faculty, p. 83 seq.

[3] Ersch and Gruber mention some 140 works upon mnemonics; see their *Algemeine Encyclopädie*, art. *Gedächtnisskunst*. Pick says that Aimé Paris gives a list of 300 works on memory and on mnemonics. G. S. Fellows, in "*Loisette*" *Exposed*, gives a bibliography of 247 works on mnemonics and the training of memory. Cf. also on the history of mnemonics, Enc. Brit., art. Mnemonics; Pick, Memory

XIII.—*Conclusion.*

The material contained in the foregoing pages, meagre though it is, may be taken as fairly illustrative of the conceptions of memory that have prevailed from the earliest times until the great era in philosophy marked by the appearance of the Critique of Pure Reason. Whether or not we agree with Emerson that all men may be divided into Platonists and Aristotelians, the various theories of memory studied naturally divide into two series—one begun by Plato, the other by Aristotle—the former transcendental, the latter physiological and empirical in its tendency. Plato, the Neo-Platonists, St. Augustine, Leibnitz regard memory as an act of the soul, limited, perhaps, by physiological processes, but not dependent upon them. Sensation may furnish memory the data in great part, yet memory belongs not to the sensory but to the intellectual part of the mind. On the other hand, according to Aristotle, Thomas Aquinas, Hobbes, Condillac, Bonnet, and others (making allowance for differences of opinion due to their individual systems), memory belongs to the sensory side of the mind. The images of memory and the imagination are the relics of former sensations. The sensations were due originally to physiological processes. The reproduced images depend upon physiological processes, weaker, but not essentially different from the original ones.

The theories of memory that we have studied may be of little value in themselves, but they form a part of the data necessary for a complete study of the psychology of memory. These theories were formed

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and its Doctors; Aretin, *Systematische Anleitung zur Theorie und Praxis der Mnemonik,* Sulzbach, 1810; Middleton, Memory Systems New and Old.

very much as we form our theories to-day, *i. e.*, by generalization from observed facts—with less of scientific rigor probably, with the usual coloring of the thinker's mental environment, and with the peculiar ornaments of the individual apperception. But, if "million-eyed observation" is better than the observation of any one man, if the experience of the race is more trustworthy than that of the individual, then a theory, though worthless as such, may be valuable because containing, however obscurely, a record of the observation and experience of the times when it was formed. Most of all, however, a theory, worthless in itself, may be valuable as an instance of the working of the human mind before one of the greatest problems of psychology. A great number of such instances may prove valuable for psychological study in the same way as the myths of savage tribes and the records of child-life.

Aristotle's doctrine of memory, for example, as a theory is partly false; but it is a remarkable instance of the tendency of the human mind to find satisfaction in resolving all mysteries back to the one supreme mystery of motion. This conception of motion played its part, too, in the systems of the Middle Ages. Hobbes made motion the basis of his system. The motion of the animal spirits was the occasion of psychic activity according to the Cartesian psychology. Memory as reproduced movement was the theory that gave most comfort to Condillac. And with Bonnet and Hartley memory is the result of persisting vibrations in nerve-substance. This theory has been taught, in one form or another, by many physiologists ever since. That so many thinkers have found the explanation of memory in motion is profoundly suggestive. The human mind has a passion for unity. If it cannot solve all its difficulties, it likes to collect them under one all-embracing mystery. This appeases the desire for unity, and economizes energy. Psychologically considered, the category of motion is an economic device that satisfies the Aristotelian mind.

Equally worthy of study are those theories that see in memory an activity independent of physiological processes, a transcendental function of the timeless and spaceless intellect.

In recent years the subject of memory has broadened. It is now connected with some of the most profound questions of psychology and biology. As the knowledge of these sciences has advanced, the importance of the study of memory has increased. Yet it is noteworthy that the beginning of the newer views is found in the doctrines of the older writers studied in this article.

THE PLACE FOR THE STUDY OF LANGUAGE IN A CURRICULUM OF EDUCATION.

M. PUTNAM JACOBI, M. D.

The study of language has always occupied a conspicuous place in educational curricula. The Greeks, who counted all languages but their own barbarian, taught the grammar of their own as the basis of all education. The Roman children studied Greek as ours do French—less as an education than as a fashion. The first mediaeval schools established grammar in the trivium, or most elementary course, and also in the quadrivium. The feeling has always prevailed in civilized communities, that as the mind was never seen to work without language, the study of language must lie at the basis of all mental training. We know now that much mental action precedes the use of words, and whenever we are logical to the laws of mental development, we train the mind to handle sense perceptions of external objects before we introduce the systematic study of language, even in reading and writing the mother tongue. Every one knows, however, that this change in the school curriculum is most recent.

The moment arrives at last when the study of language must begin, even if nothing is learned but the native language of the child. This moment may to a certain extent be compared with that illustrious epoch in European history, when at the Renaissance of learning, classical Latin and Greek were rediscovered for the modern world. The extraordinary effect of this discovery may well serve to prove the importance of language to thought. With an imperfect and inadequate language, the nations of Northern Europe had remained in a narrow, cramped, and as we now often say, with perhaps considerable exaggeration, a barbarous existence. Restored to the noble speech of which they were the just inheritors, their compressed life rapidly expanded to its measure. The new vitalities aroused, soon in turn expanded the hidden potentialities of the antique tongues to all the flexible and varied needs of the modern life, and this life rapidly developed to a hitherto unknown degree of complexity. An immense number of thoughts seemed to have been impossible from the lack of fitting words. When these words were found—the buried treasure of bygone ancestors—the thoughts sprang to them as rider to the saddle; and with new ideas, life was regenerated.

Thus, although the material for the physical sciences existed in the same abundance then as now, these sciences failed to develop until after the Renaissance of classical learning. It seemed necessary that Scaliger and Erasmus, in the sixteenth century, should precede Gilbert and Harvey in the seventeenth, to render possible their discoveries of electricity and of the circulation of the blood. The solitary labors of Roger Bacon in the thirteenth century had flickered like a taper in a vast cavern of darkness, and then failed for lack of air. The human brain could not advance in analysis of the external world until it had been disciplined and developed in its internal activity by training in language.

But, at the present day, the educational value of the study of languages has begun to be seriously questioned. In a late number of the *Forum* Dr. Flint declares that as much mental discipline can be obtained from study of physics and chemistry as from study of languages, and that the knowledge thus gained is both more useful and more easily understood than the construction of Latin and Greek. He also observes that the range of subjects on which knowledge is desirable has greatly widened since the classical curricula were planned, and that it is impossible to do justice to all that is necessary to-day if we continue to fulfil all the demands which were made two hundred years ago. Similar remarks are repeated over and over again, and on all sides. These assertions touch, indeed, upon some truth, but they do not comprehend all of it, and they overlook much that is essential to the questions at issue. The problems to be considered are:

1. Does the study of language exercise any different effect upon mental development from the study of any other subject, and if so, what is it?
2. How does the effect of language study compare with that of mathematics, of the physical sciences, of the moral and historical sciences?
3. If such special effect can be proved, at what age or epoch of education is it most appropriate and useful to seek for it?
4. Is there any difference between the effect on the brain of the classic and the modern languages?
5. If languages are to be taught, how is the necessary time to be secured for teaching other things most important to know and too often neglected?
6. What proportion should these relative branches of study bear to each other in a general, non-specialized curriculum?

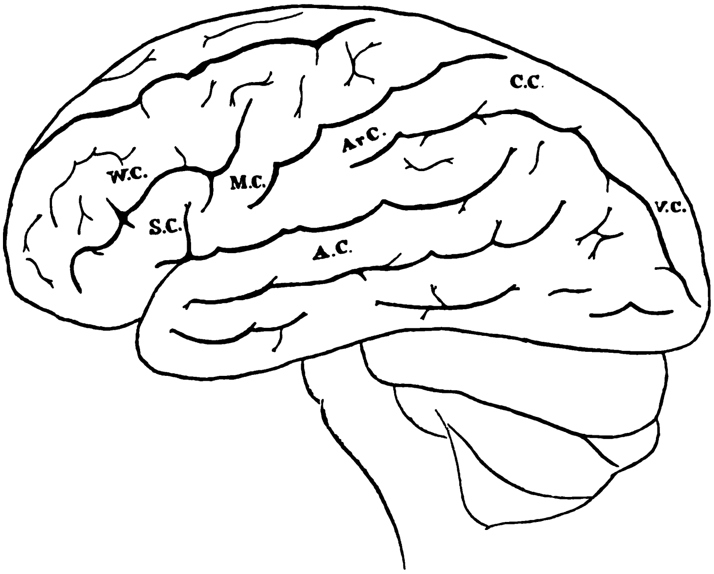
7. What special devices or methods may be suggested to facilitate the accomplishment of the above mentioned ends?

At the outset I would call attention to a fact which might seem self-evident, yet is generally overlooked in pedagogical discussions of the subject. This is, that the study of languages must be an extension, more or less complex, of the process of acquiring language—the highest physiological acquisition that distinguishes the human race from the lower animals. The method and educational results of such study are, therefore, primarily a physiological problem, and should be discussed by physiologists before they are handed over to pedagogues.

The genesis of speech is one of the most extraordinary and mysterious phenomena in the history of mankind. It has always justly excited the astonishment and speculations of philosophers.

It is most difficult to understand why any particular sound, or group of sounds, should have become significant of one object or idea rather than of any other. A purely physiological theory has tried to classify all words according to the parts of the articulating apparatus at which their fundamental sounds are formed, thus giving one intrinsic meaning to guttural sounds, another to labials, another to dentals, etc. But this theory cannot meet all the facts of the case. Prof. Max Mueller, who traces all words in the Indo-European languages back to 850 primary Sanscrit roots, is inclined to accept another physiological explanation of the genesis of the roots. This is the theory of Noiré. The latter has pointed out that whenever a number of people are engaged together in any muscular work, they have a tendency to utter aloud certain rhythmical sounds. "These are almost involuntary vibrations of the voice, corresponding to the more or less regular movements of the whole bodily frame." Noiré suggests that some special nerve element, or group of nerve elements, in the brain is then thrown into vibration coincidently with the external muscular movement; and this associated nerve vibration being propagated to the part of the brain which innervates the organs of articulation, the latter are excited to so modulate the simultaneously developed current of air in expiration, that a definite sound, one of the primitive root sounds, is produced. This verbal root remains associated with the act which was being performed during its articulation, and finally becomes an expressive sign for the entire class of acts during which it is habitually repeated. "Thus would be explained," observes Mueller, "the fact that the primitive Sanscrit roots all express actions and not objects: as actions of digging, cutting, rubbing, etc." Words expressive of other ideas are derived from the first by analogy and metaphor. This theory should also explain why any given root should bear a special relation to any given action, and hence come to express any special group of ideas. It does so, because it has been generated in a cerebral excitation, that has happened to coincide with such other cerebral excitations as have been necessary to the performance of voluntary muscular actions.

Thus, in the figure, *Ar. C.* is placed on a part of the brain that we know is always excited when a person is using his right arm; *S. C.*, on a point very near it, which is always excited when he is speaking. Nerve impulses pass from this point down through the brain until they reach the nerves coming from the base of it, and



W. C. writing centre, *S. C.* speech centre, *M. C.* motor centre for lips, *Ar. C.* arm centre, *C. C.* concept centre (location hypothetical), *A. C.* auditory centre, *V. C.* visual centre.

which go to the throat, tongue, palate and lips. According to the theory, the excitation or vibration of nerve elements at the point *Ar. C.* spreads to point *S. C.*, the so-called centre of articulation, where it throws nerve cells into some special form of vibration. This special form of vibration is transmitted out of the brain, along nerves going to the lips and other organs of articulation, and the current of air which is at the moment issuing from them is moulded into some special articulate sound. This becomes a root, an auditory sign, which first evolved (according to the hypothesis) during the performance of a given act, is repeated with every repetition of the act, and gradually becomes an abstract sign corresponding to the generalized conception of such a class of acts. Thus the first abstraction of speech would result from a generalized experience of a succession of personal actions. In the second stage of development, the sign would be extended by analogy to other actions than the original one; finally to the properties of objects that seemed explicable by reference to these actions, which were better known than the objects themselves.

Thus, observes Mueller, every root expresses a concept or general notion, or more correctly, the remembered consciousness of repeated acts, as scraping, digging, striking, joining, etc.[1] As a single illustration. From a root *khan*, to dig, easily came *khana*, meaning not only a digger, but also a hole ; and *khani*, a digger and a mine.

I will not dwell on the various interesting facts which might be adduced in support of this theory. But, in considering it, we are led to note the fundamental circumstance that speech implies a more extensive excitation of the brain than does any action performed without speech, including in the latter the systematized thinking which clothes itself in words. In its most rudimentary form, the articulate utterance accompanying a muscular movement implies that nervous action has spread from the nerve centres governing the movements of limbs, to those adjacent centres which control the organs of articulation. Closely adjacent to these centres are other portions of the brain which have no immediate connection with nerves either going to or coming from the brain. The Island of Reil is one of them. These portions of the brain are concomitantly drawn into the vortex of excitement, and when that is the case, the vibrations of nerve cells and fibres which occur during the utterance of the speech, are repeated or registered, as it is said, in these extra-sensory centres. It is then, in some mysterious way, that the consciousness or conception of speech is generated in the brain and mind of the speaking individual. The genesis does not occur unless the supra-sensual, superadded convolutions of the brain have attained a high degree of development, and this is why no animal but man is able to speak.[1]

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[1] The Science of Thought, p. 214.

When any one learns the terms of a fully developed speech, or a baby learns his own language, the process is different. Here is no question of generating a spoken sign, compelled to assume an indissoluble relation to some thing. But it is only necessary to learn the spoken sign already created, and the fact that it *is* associated with a thing.

The sound of the word, as bread, falls upon the air and causes a peculiar vibration of the nerve running from the ear to the brain—the auditory nerve. This vibration is transmitted to a special locality of the brain, apparently the first temporo-sphenoidal convolution. Now, if the child has never seen any bread, the sound, though registered, arouses no mental conception ; it seems to have no meaning. It is the same when an adult hears a word in a language to him unknown, or when the subjects of certain forms of brain disease hear words after they have lost the power of attaching any significance to them. But if the baby—to return to him—has seen a piece of bread ; if he has become sufficiently interested in it to notice the association of this verbal sign with it ; if the association has been distinctly pointed out to him, by pronouncing the name at the same time that the bread is shown or given, then another process takes place in his brain. At the same time that the name is registered in this part of the brain, the receptacle for auditory impressions, a visual impression of the object is registered at another point—the cuneus, or posterior portion of the occipital lobes. Often, indeed, the visual impression has been made long before; the child has recognized the appearance of the piece of bread, when it could not as yet name it, but only reach after it with a gesture.

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[1] See the most interesting paper of Broadbent on "Cerebral Mechanism of Speech and Thought," Med. Chir. Trans. 1872.

When the two impressions have been registered in the brain—the visual impression of the object, and the auditory impression of its name—they may then be combined. Exactly how this combination is effected we do not know; but we can represent to ourselves that vibrations, similar to those of the auditory nerve, are transmitted along the fibres which connect these two points of the brain. When this happens, a secondary vibration is coincidently transmitted in another direction to the convolutions "superadded" to the simplest ones which belong to the sense impressions. In these convolutions the more complex combined vibration becomes the material correlative of an ideal concept, composed of the reminiscence of the visual impression of the object and of the auditory impression of its name.[1] Taine remarks that a couple is then formed, either member of which is thenceforth able to draw the other into consciousness. The sound of the name suggests the image of the object; the sight of the object suggests the sound of the name.[1]

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[1] L'Intelligence, p. 6. The precise statement is as follows: "In the formation of *couples*, such that the first term of each suggests the second term; and in the aptitude of this first term to stand wholly or partially *in place of* the second, so as to acquire either a definite set of its properties or all those properties combined, we have, I think, the first germ of the higher operations which make up man's intelligence."

The association of written signs with visual images and with auditory signs is obviously only an extension of the same process, and need not be dwelt on here.

The child learns to recognize a word before he learns to use this word himself; but finally this step also is taken. He articulates the word "bread" under the influence of an internal impulse or desire composed of the sensation of hunger, of the reminiscence of the visual impression of the object, of other impressions or memories connected with it, as of its hunger-satisfying property; finally, of the auditory impression of its name. This complex internal impulse, when definitely formulated, corresponds to an excitation of some part of this intermediate portion of the brain that we may call for convenience, as it has been called, the concept centres. From these centres the excitation spreads to that point, whence are innervated the organs of articulation; and when they are excited in the proper way the child is uttering the word "bread." By that time an entire cycle of cerebral activity has been traversed, and the greater part of the area of the brain has been excited. *It is plain, therefore, that to learn the name of a thing, and to learn how to use this name, involves much more mental action than is required simply to acquire sense perceptions about it.*

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[1] In an interesting paper on Apraxia and Aphasia, by Dr. Allen Starr (N. Y. Med. Record, Oct. 27, 1888), the hypothesis of a "suprasensual combining centre" is pronounced superfluous. The combination of the visual and auditory impression is said to be *virtually* effected when these simultaneously exist in the brain, and hence in the unity of consciousness of the individual.

The name, moreover, constitutes an important rise above the level of sense perceptions, and marks the initiation of a process that is to lead to all abstract thought.

The second step in this process is taken when the name of a single object is generalized to others so as to form a class.

Taine tells a pretty story of his little girl to illustrate these early efforts at classification. She had learned to call a lamp "brûle," and was also in the habit of playing hide and seek with her nurse, with the exclamation "cou-cou" uttered as the nurse's head disappeared behind her apron. The first time the child saw the setting sun she exclaimed, "a brûle cou-cou!" The new object was brilliant like the lamp, and disappeared like the nurse's head. The child imitated the logic of her Aryan ancestors, in combining this new double experience into a single expression containing the two characters of each of the others.

Thus the second step in language is a process of generalization by means of observed analogies. Between individual objects a complex mental concept is formed, existing nowhere in nature, but only in the mind of a human being holding it. In the act of extending an individual name to a class, the little child passes out of the animal world into the human world; he becomes a rational being. For this reason some thinkers, as Professor Harris, have held that the possession is not only the sign of the soul, but the demonstration of its immortality. Whether this be so or not, the possession is none the less marvelous.

When verbal signs have once become associated with objects, it is possible for the mind to occupy itself exclusively with them, and altogether to disregard the objects. It is as with signs of number, with whose aid, most complicated operations can be performed by the mathematician, which would be quite impossible if he were obliged to handle the concrete material objects to which these signs originally referred.

By means of signs, verbal or algebraic, the mind emancipates itself from things; by analogy and metaphor and combination, it contrives to clothe the suggestion of a single root, with endless successions of meanings, among which the original significance may be entirely forgotten.

Thus the fundamental fact in the acquisition of language is, that it arouses the activity of the highest centres of the brain—the ideal or concept centres without whose functions all knowledge of the external world must remain as isolated groups of sense impressions. Language is essential to all but the simplest forms of thought, because the registration in the brain of a combined impression or personal experience, derived from the union of two or more sense impressions, is always attended by such a diffusion of excitation to the speech centre, that the organs of articulation are called into play, and words are pronounced. This at least is the case while speech is being generated or acquired for the first time. Subsequently, the utterance of speech aloud may be restrained; but none the less is the speech centre thrown into activity, and the word re-echoes in the brain to the footfall of the thought.

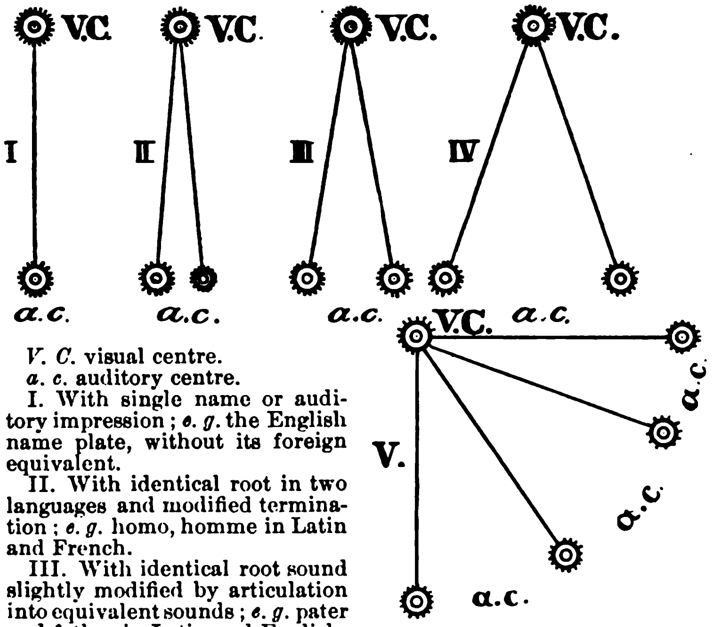
The acquisition of foreign languages modifies the cerebral processes just described, by rendering them even more subtle and complicated.

The nervous tissue of which the brain is composed, and to whose structure I have summarily alluded, is composed of an immense quantity of microscopic cells, traversed by delicate fibres, connected with each other by fine fibre-like prolongations of their own substance. By means of somewhat coarser fibres, separate territories of cells, and cells and groups of cells and fibres in the brain, become grouped together. It is because of the possibility of infinite variety in these groupings that the possibilities of speech are practically infinite.

The registration of a spoken word involves, we may say, schematically or provisionally, the excitation of as many nerve cells in the auditory centre as the word is composed of separable sounds. Thus, the word "father" implies two distinct excitations, one for the sound "fa," and the other for the sound "ther." Let us suppose now that another auditory impression be made, for the same object, by its Latin name, pa-ter. This name does not only correspond to the same object; it is philologically identical with the English word, the Latin being merely a modified articulation of the same root and termination. If therefore, having pronounced the syllable fa, we then pronounce the syllable pa, we must infer that the brain of the person perceiving the difference, registers the second syllable in a different, but closely adjacent locality to that registering the first; we may suppose, in the very next nerve cell.

If an object be successively described by two names whose sounds are not identical and which are derived from different roots, then we must suppose that not only different nerve cells, but different, and perhaps rather widely separated groups of nerve cells receive the auditory impression. Thus the English and German names, man and Mann, identical with each other, are entirely different from the Latin and French homo and homme, which are identical. The nerve territories impressed are not therefore adjacent, and when the double sets of verbal signs for the four languages become associated in consciousness with the same object, we must suppose that the impulses converging upon the visual centre, to combine with the visual impressions of the object, are gathered from a larger area than when only a single auditory sign has been used.

The area is still wider if there are four entirely different words in the four languages known. The different conditions in the four cases may be represented thus: (it must be remembered that whenever two distant regions are affected, the fibres connecting the two must also be modified.)



V. C. visual centre.
a. c. auditory centre.
I. With single name or auditory impression; *e. g.* the English name plate, without its foreign equivalent.
II. With identical root in two languages and modified termination; *e. g.* homo, homme in Latin and French.
III. With identical root sound slightly modified by articulation into equivalent sounds; *e. g.* pater and father in Latin and English.
IV. With entirely different names or root sounds for the same object in two languages; *e. g.* mensa and table in Latin and English.
V. With different root sounds for four languages.

Adopting the convenient schematic representation of the cerebral process involved, as a vibration and combination of vibrations, we may compare the successive complications to the vibrations of piano strings combined as follows:

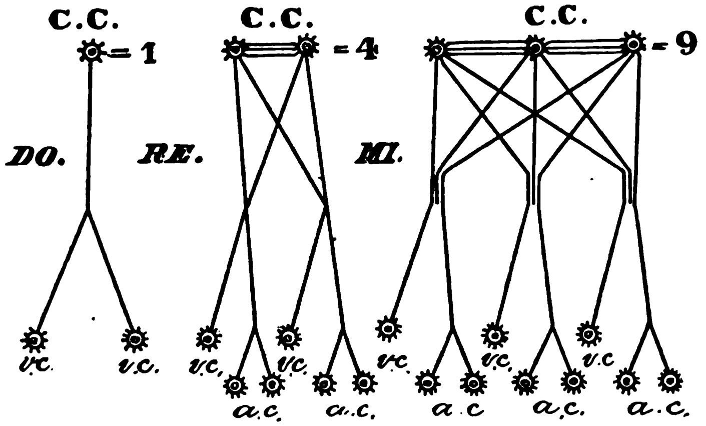
I. Tone A combined with tone B.
II. Tone A combined with tone B and semitone C.
III. Tone A combined with tone B and tone C.
IV. Tone A combined with tone B and tone G.
V. Tone A combined with tones E, F, G, in another octave.

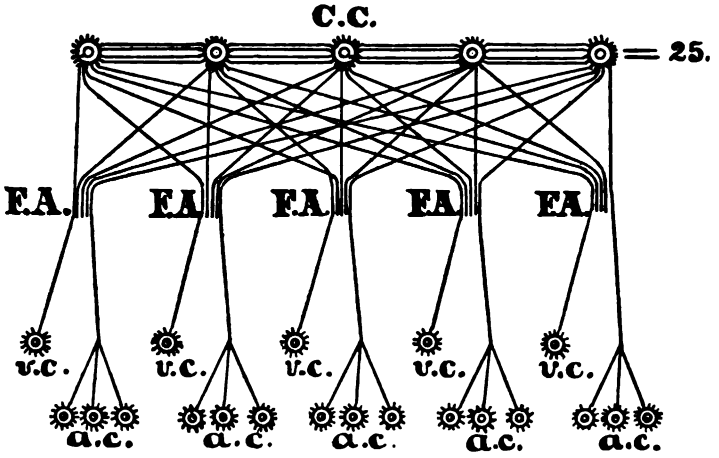
The various combinations and extensions of the area of cerebral excitation are effected even while the process remains limited to the instinctive acquisitions of multiple verbal signs, *i. e.* of two or more languages, by such unconscious effort as a child expends in learning his own or a foreign tongue in the nursery. But in the deliberate study of several languages, the complex combinations effected between the visual and auditory centres are carried up into the ideational or concept centre, there also to widen the area of excitation and increase its complexity.

To illustrate: let us call the first combination above described, AB; the second, ABc; the third, ABC; the fourth, $ABCDEFG$; the fifth, $AB+$, representing extension to another octave, with affection of all the intermediate notes.

These combinations may then be indicated by single symbols, which, to continue the analogy with musical vibrations, we will take from the musical scale: $AB =$ Do; $ABc =$ Re; $ABC =$ Me; $ABCDEFG =$ Fa; $AB+ =$ Sol.

The more complex the vibration transmitted to the concept centres, or to those parts of the brain where visual and auditory impressions and their combinations rise into consciousness, the more complex the excitation which will be produced at these latter centres. This is equivalent to saying that there will be a more complex and varied generation of ideal impressions or ideas, by whatever mysterious process that may be brought about.





Thus, in this diagram, let *C. C.* represent the concept centre, thrown into vibration by impressions produced during the various combinations of the excitations of the visual and auditory centres, *V. C.* and *a. c.*

The diagram is intended to illustrate in a rude and approximate way, how the more complex vibration, as represented by the symbols, affects a larger area of all the groups in the concept centres, and also how the intensity of nerve vibrations at these points must rapidly increase. The increasing intensity is represented by the figures 1, 4, 9, 25. The series could of course be indefinitely extended, yet must always fall infinitely short of the complexity of the actual process.

To sum up: the acquisition of foreign languages in addition to the native tongue multiplies the number of verbal signs which the mind habitually couples with visual impressions. In registering and in using these multiple signs, the mind is compelled to more complex operations than when only one sign is used. When in different languages different primary words or roots are used to represent the same object, then the mind, using them all, becomes acquainted with the several aspects of that object which have impressed the minds of those among whom these different names have sprung up. Thus a larger impression of the object is formed, and the mind of the speaker, which is rendered more flexible and active by engaging in more complex internal processes, is also enlarged by a richer store of external impressions. This latter effect is proportioned to the degree to which the different language aspects of the object are thoroughly studied; it may be entirely missed if they are not deliberately studied at all, but words learned only by rote or by habit.

It is finally to be noticed that while the mental or cerebral process increases in extent when multiple names are learned which have no relation to each other, *i. e.* which come from entirely different roots, the delicacy and finish of these processes is more increased by the study of closely related words, *i. e.* those with precisely the same root and only modified termination, or those whose identical roots are modified by the introduction of equivalent sounds, as p for v, g for k, etc.

The reason for this is the same as for any nervous action, and is conspicuous in nervo-muscular actions. Every one knows the immense superiority in delicacy and subtlety of the movements performed by the fingers as they pass through minute areas of space, as compared with the movements of the arms or legs, which may extend so much further. And in a similar fact lies the reason for the immense mental discipline to be derived from the study of the European languages, which are all so closely related as to be scarcely more than cognate dialects of Greek, Latin, and Gothic. The discipline is only obtainable when these languages are studied together as simple varieties of the European language. To study them separately and successively is as illogical and time-wasting as it would be to concentrate isolated attention upon peach blossoms or plums, instead of considering at once the great rose family, of which they are members. Neither in the botanical nor in the philological family can the characters of genera and species be understood without incessant reference to the more general characters of the class to which they belong. This reference is even more important for languages than for plants, on account of the incessant transformation of the one into the other, and of the historical phenomena of development and decay which they share in common with living organisms. In the attempt to acquire an empirical acquaintance with apparently unrelated facts, enormous amounts of time are wasted, which would be saved by the scientific insight into the real relations of these facts, with which the study might just as well have been begun.

The first question we have proposed for solution may now be answered thus. There *is* a special effect produced by the acquisition of language, so special that it serves to distinguish man from the brutes. It depends upon and incessantly develops the ability to use abstract signs as symbols of things, and to use them apart from these things. It is essential to the elevation of the mind above the level of sense perceptions; and itself develops the mental sphere in which ideal conceptions arise, combine with one another, and generate endless successions of new ideas.

The process of acquiring foreign languages, in addition to the mother tongue, modifies the original process, by extending, refining, and complicating it. Impressions are immensely multiplied and the mind becomes accustomed to take cognizance of such subtle differentiations that its delicacy of perception is indefinitely increased. The capacity to appreciate subtle distinctions, more subtle than those existing in nature outside of the mind, is essential to scientific work. It is also essential to a high grade of ethical culture. Not unjustly have language studies been entitled "Humanities"; for it is the grade of mental development which they foster, that is necessary for the harmonious and finely equitable maintenance of social relations. Without this culture, the study of the external world, even if successfully pursued—which is rarely the case—is liable to have a materializing and even brutalizing effect, and that in proportion to the complexity of the interests involved. It is very possible for an illiterate carpenter to be a very honest fellow; but it is much more difficult for an illiterate physician to be truly honorable, even when skillful in his craft.

II.

Language is not the only abstraction to which a young child becomes accustomed. The abstraction of number comes to him very early, and the study of arithmetic should even precede the systematic study of language.

Our second question demanded a comparison of these two forms of abstraction, Language and Arithmetic. The comparison is not difficult to make. Number is a single quality abstracted from objects, to be handled separately by means of its signs. But words represent multiple qualities combined in constantly varying proportions.

When the child first learns the principles of number, it must not abstract this quality from concrete objects; but these are to be handled until a number of concrete visual impressions have been firmly engraved upon the mind.

With words, however, the association with visual impressions, which is so much more complex, must also be maintained for a much longer time. For two or three years, no word should be given to the child or handled by him which cannot be directly referred to sense perceptions; and it is indefinitely desirable to revive their association and to make it as vivid as possible.

Thus, mathematical signs, earlier detached from objects, soon pass into a more purely abstract region than words, from which the image of the object is never completely effaced, and which indeed constitute forever a marvelous transition ground between purely mental conceptions and purely sense impressions. The high degree of abstraction of mathematical signs, however, is balanced by the much greater simplicity of their mutual relations; while the more concrete and sensuous character of verbal signs is associated with an incalculable multiplicity and qualitative variety of interrelation. Hence they bring the mind much nearer to the infinite variety of nature than does mathematics. The abstractions of language prepare for the copious details of natural science and of practical life; the abstractions of mathematics, though essential to the scientific manipulation of these details, are liable, if uncorrected, to unfit the mind for their assimilation. Mathematical training facilitates the working of the syllogism; but language training tends much better to facilitate the discovery of the premises.

Let us now compare the study of language with the study of physical science.

Physical science consists of two parts: 1st, the acquisition of sense impressions through contact with external phenomena. 2d. The collation, comparison, and classification of these impressions, reasoning upon them, and establishment of the laws of phenomena.

The first process collects the raw material of science. But it is the second process that creates science out of its raw material. Science is not nature, but the product of the mind acting upon nature.

Thus the first process in scientific study corresponds to the activity of sense impressions, which for every individual constitutes the earliest form of conscious activity. The second process corresponds to the second step, taken when the mind reacts upon its sense impressions sufficiently to generate words, to create language. Words are the first products of the action of mind upon nature, as science is the latest and most complex expression of the same action. Thus language is the earliest and most perfect type of science. In its three-fold nature it offers a three-fold type, namely, in words, in grammar, and in literature.

Words, as has been shown, result from the combined activity of several sensory centres in the brain, taken together, or further combined with that of its ideal centres, the latter being, probably, portions of the brain which are not immediately connected with sensori-motor apparatus or with sense impressions. Words may, therefore, be compared to the centaurs of antiquity which were half man and half beast. For on one side they contain the image of external objects ; on the other, they consist of a mental sign which has been generated within the brain. Hence, words may be studied in a twofold manner, objectively by methods appropriate to any study of objects, while subjectively they may be utilized to exercise the mind in handling abstractions not yet disconnected from concrete things.

Now, it is quite impossible permanently to choose, as some people seem to imagine, between study of words and study of things, after the very first steps have been taken. The first steps must certainly consist in direct observation of things, and in training the senses by such observation. This doctrine is very recently enunciated, but now commands general acceptance. We know now that the use of language does not indicate the first activity of the mind, but the second.

Education should not, therefore, begin with language, with the alphabet, and reading and writing, any more in the mother tongue than in a foreign language. It should begin with the systematic training of the sense activities that occupy the first six or seven years of life and alone are consciously exercised at this time; the growth of speech, though proceeding with marvelous rapidity, being a quite unconscious process. I have said elsewhere that a child who is taught words before he has learned to handle things is liable always to rank things in subordination to words, a dangerous and often fatal error. But in the handling and observation of things by a young child there soon comes the necessity for a pause. The necessity depends upon two circumstances ; the material to be studied is difficult of access, and its important properties are too complex and too recondite to be made appreciable to the child's senses, consequently not at all to his mind. Because a simple sense perception is possible to a child at the time that a complex mental relation would be incomprehensible, it does not follow that a complex sense perception is more easily appreciated than a simple mental relation. Still less does it follow that it is possible to convey to a child knowledge of many of the most fundamental facts of science, which are not merely phenomena of nature, but complex ideas, composed partly, indeed, of observations of phenomena, but partly also of the inferences, often very subtle, which have been based on these observations.

It is a most ludicrous misconception of the nature of science to suppose that the little manuals and primers which abound for the purpose of disseminating information apart from scientific method, really teach anything at all. Again, it is a most dangerous preparation for the study of science to call upon children to imagine or represent to themselves facts which have not been apprehended by their senses, or those which could never be. Why should we try to make a child believe that the earth goes round the sun, a statement which contradicts all the experience of his senses? I should rather tell a child, if interrogated, that I have heard that some people said so, but that I myself had no real knowledge on the subject; which is strictly true. Scientific imagination is only permissible to those whose minds have once become saturated with pictures of real things from prolonged contemplation of nature. The interposition of drawings, schemes, models, diagrams, and the like does not facilitate knowledge of nature, but tends rather to fatally defer the possibility of attaining this knowledge. Hence, until the real objects can be perceived, and by means of the real scientific methods, there is nothing gained, but only precious time wasted in pretending to study them. This same precious time can, however, be utilized in the study of a class of objects which are everywhere accessible in abundance, and whose properties can be rendered conspicuous and intelligible to a properly prepared child of seven or eight. This class of objects consists of words.

There can be no antagonism between the study of things and the study of words; but the first must initiate education, and the second take it up when further progress in the first has become too difficult. To the study of words, as I propose to show, may be brought the scientific methods used in the study of things—observation, analysis, comparison, classification; and the child may thus begin to be trained for physical science at a time when the pursuit of most physical sciences is impossible.

The purely descriptive sciences of botany and map geography, already begun, may indeed be slowly pursued; but the most strenuous study for the time should be that of language. This study does not merely serve to occupy the time and to acquire a kind of knowledge necessary for practical purposes at a time when such acquisition is most convenient. But it provides, even in its first stage—the study of words—a discipline that is quite indispensable to the pursuit even of physical science, whose alleged utility is so often contrasted with that of language.

The habit of handling abstractions, if not exactly essential to the simplest perceptions, is essential to all thought about these perceptions. It is essential also to all perceptions beyond the simplest and most obvious, for the larger part of what the mind perceives is what the mind brings to the object from its previous store of knowledge and reflection. Every word is a condensed generalization of experiences or of observations. Only those accustomed to words are successful in condensing into unity even their own observations; still less, those of multitudes of other people.

The second part of language, grammar, affords still higher training in the mental processes involved in scientific study. Grammar is the science of relation between conceptions. It is the science of propositions, of the laws whereby words so group themselves in consciousness as to form distinct complex ideas. We have supposed that individual impressions depend upon the excitation of definite areas of brain tissue, and that verbal impressions were peculiar in resulting from the combined excitation of several such areas.

A proposition implies the coincident excitation of a much larger number of areas, and especially in the non-sensory concept centres. The physical basis of the relation of the parts of speech in a sentence to each other we must represent to ourselves to be the vibration of the fibres (the associating fibres of Meynert), which connect these several excited areas and bring them into material relation with each other.

The study of grammar, therefore, differs from the study of words in two ways. It calls into play more predominantly the concept centres as compared with the sensory centres; and it emphasizes the excitation of the connecting fibres of the brain rather than that of the ganglion cell areas which they connect. Grammar, which from a certain standpoint is justly considered to be a branch of logic, disciplines the brain in handling and grouping the impressions which have been registered on it. The discipline thus obtained prepares the mind to similarly group and handle all new impressions; prepares it, therefore, to find a discipline in the material of physical science, as it could not otherwise do. Without such previous training in language, the mind is almost inevitably staggered and confused by the immense mass of impressions it tries to grasp in either physical or moral science.

Literature, the third department of language, represents the action of mind upon nature in a manner co-equal with that shown in science. To enable adolescents to become acquainted with European literature, it is necessary that in childhood the preliminary work in the lower departments of language, words and grammar, shall have already been accomplished. In words and grammar are already found outlined or reflected the history and the philosophy of European nations. Studied with the same system and method that would be applied to the material of a physical science, words and grammar will lead the child insensibly, but profoundly, into the very heart of literature, and into the central life of the races of humanity that concern him. Until he has touched upon this, his own is incomplete.

The foregoing considerations answer, we think, the second question, which asks a comparison between the educational values of language, mathematics, and physical science.

They also answer the third question, namely, when the study of language may be most profitably pursued. The characteristic time for this study is between the age of seven, as the kindergarten training closes, and the age of fourteen or fifteen, when really scientific studies may be begun.

III.

I have asserted a little while ago that the most characteristic benefits to be derived from the study of European languges are only obtainable if several of them are studied simultaneously, and on the same plan with which we should study the different members of a single botanical family.

The table below shows the division made by modern philologists of the great Indo-European family of languages. Out of these it is sufficient, both for practical and theoretical purposes, to select three branches, the Greek, Latin and Gothic branches. From the first two we need Latin, Greek, and French. From the third, English and High German. Knowledge of these five languages is requisite to the real understanding of any one of them; and if these are possessed, knowledge of the remainder, though often most interesting, is unessential and may be deferred or neglected. Thus as a modern representative of Latin, either French or Italian, perhaps even Spanish, might be selected; but on the whole, to-day, a practical acquaintance with French is most often required; and, as Milton observed, any one who knows Latin should be able in three weeks to learn Italian. It is hardly necessary to observe that these languages contain the literature and mirror the thought and life of Europe. Nor is it necessary to dwell on the vulgar error which would distinguish Latin and Greek as dead languages, and hence less useful than modern dialects which may possibly be spoken. To an English-speaking person of any culture, Latin and Greek are far more living than Spanish or Portuguese or Dutch, all spoken languages. Five-sevenths of our English vocabulary is Latin.[1] As Prof. Harris remarks, we are still living in the midst of Roman civilization. Yet Greek is so much nearer the complex flexibility of modern habits of thought, that Dr. Schliemann might almost be justified in urging its

INDO-EUROPEAN LANGUAGES.

ARYAN.	SOUTHWEST EUROPE.	NORTHERN EUROPE.	
		SCLAVONIC.	
		Bulgarian.	Bohemian.
SANSCRIT.	GREEK.	Polish.	Lithuanian.
	Modern Greek.	Russian.	Old Prussian.
		TEUTONIC.	
IRANIAN.	LATIN.	Gothic (extinct).	
	Italian.	Scandinavian.	
	Spanish.	Danish.	
	Portuguese.	Swedish.	
	French.	Norwegian.	
ZEND.		Icelandic.	
		Germanic.	
	KELTIC.	Low German.	High German.
	Tribes in Spain.	Friesic, Dutch.	English, comp.
OLD PERSIAN.	Gaul.	Anglo-Saxon.	from
	Britain.	Old Saxon.	Anglo-Saxon.
Armenian.	Ireland.	Low German.	Latin.

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[1] Whitney: Life and Growth of Language, p. 117.

acquisition before Latin, and as a spoken conversational tongue. It is, moreover, as is just beginning to be noticed, a really modern and still spoken language; but this consideration is practically less important than the others adduced.

With which vocabulary from among these languages a child begins his systematic study of language, is almost a matter of indifference. Still, it is usually preferable to select Latin, because its letters are the same as English, as is not the case with German and Greek; because the structure of its words and spelling is most closely allied to English, which is not so obviously the case with French, whose pronunciation also offers peculiar difficulties; and finally, because the regularity and simplicity of its grammar render it the language in which the principles of grammar should first be studied. Greek grammar is more complex; French and German, more arbitrary and capricious, especially French. English grammar is atrophied, and as unsuitable as a field wherein to learn the principles of grammar, as the hoof of a horse would be as a model for the study of feet.

It is desirable, when possible, that a child learn instinctively two languages from birth; but it is also desirable that no attempt be made to teach it to speak more than two. Supposing these two languages to be English and German. At the age of six and a half or seven, a dozen lessons should suffice to initiate the child into reading the same, when he is only obliged to translate the new visual signs into auditory signs with which he is already familiar. The initiation once effected, it is quite unnecessary to pursue further special systematic instruction in reading and writing these two mother tongues; knowledge of which will be picked up incidentally, and much faster than by the usual methods. But the child may at once, at the age of seven, begin to read in Latin and French simultaneously. It is not customary to consider this possible, because the study of foreign languages is habitually initiated by the study of their grammar. But this is as unphilosophical as was the former practice of beginning the study of English with spelling and grammar.

Children tend to learn a foreign language by precisely the same process by which they acquire their own. They first learn words, and are so powerfully impressed by the roots of these, which convey all their essential meaning, that they remain perfectly indifferent to their collocation, termination, and inflection. If, disregarding this natural tendency, the teacher compels the child to study grammar first, an opportunity to learn a great deal is wasted, and much time is also wasted in learning a very little.

Part of the mistake depends upon the assumption that a child must be taught to speak the language before learning how to read it; and for speaking correctly, a knowledge of grammar and idiom is indispensable. This is the view taken of the modern languages. But another mistake is made when Latin is considered; for as a really fluent reading knowledge of Latin is to-day rarely aimed at, the advantage of its study is often supposed to lie exclusively in the discipline afforded by its grammar. Hence, with French, the child is tied down to endless uninteresting questions about the umbrella of my aunt and the inkstand of my grandmother, in the useless attempt to teach him to speak French correctly; or in Latin is drilled upon the galloping of swift legates from the armed city, so that he shall be able to parse Caesar's Commentaries. Yet I imagine that even Roman children did not trouble themselves much about legates. And the conversational methods of modern French text-books, often admirably designed when the time has really come to teach grammar, will, when premature, only serve to suggest to the child, as I heard one say, "that the French must be an awfully inquisitive people to ask so many foolish questions."

The manipulation of a foreign language by speaking and writing it to express one's own ideas is a much greater cerebral effort than is generally recognized. It is an effort that is not demanded at the same stage of knowledge about any other subject. For instance, a student is expected to spend a very long time upon the study of descriptive botany before he would be called upon to invent botanical theorems of his own. Speaking a foreign language is the mental equivalent for thinking out original propositions in a foreign science. The difficulty is usually evaded by the student using some hybrid form of speech, as Roger Ascham long ago remarked was the case with young English children compelled to speak Latin,—or rather in a barbarous gibberish that rather deferred than facilitated their acquisition of the classic speech.

It is the study of words, which corresponds to the descriptive study of the details of a science, with which the mind must become saturated before it attempts to re-arrange their relations into new formulae. It is the study of words, therefore, which should come first—*not* the attempt to use them, except where the language has been learned instinctively in the nursery.

The words cannot certainly be learned in rows out of a dictionary, but only in connection with their context.

For Latin it is well to construct simple sentences containing only a subject, object, and verb in the third person, which sentences the child must be shown how to read, translate into English, and then write out a translation into French. This can be done at the very moment the child is still learning how to read in English, and an immense amount of time thus be saved. A three-fold impression is made upon the mind; the words in the three closely allied languages fuse readily into a complex conception, which retains its several parts much more firmly than when each is learned separately. At this epoch the mind is naturally quickened for the acquisition of verbal signs, and the acquisition of one facilitates that of the rest.

When any set of mutually convertible sentences has been written in the three or four languages, the words in them may be picked out and their roots compared with one another. At seven years old it is quite easy for a child to learn to understand the nature of roots. In his own use of language, as has been said, a child cares for nothing else. He is very much in the condition of his primitive Aryan ancestors. Remembering the fact that to a child anything may be made intelligible which is appreciable by his senses, it is clear that there should be no difficulty in pointing out to him the affinity of the sounds produced by the same organs of articulation. He can be easily taught to distinguish gutturals, dentals, and labials, or even the distinction of surds and sonants, and thus to learn the facts at the basis of Grimm's law.[1] In the table below is shown

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[1] GRIMM'S LAW.

	Original Sound.	Sanskrit.	Greek.	Latin.	Gothic and Low German.	High German.
Aspirates	kh	gh (h)	χ	h, f (g, v)	g	k
	th	dh (h)	θ	f (d, b)	d	t
	ph	bh (h)	φ	f (b)	b	p
Sonants	g	g (i)	γ	g	k	ch
	d	d	δ	d	t	zz
	b	b	β	b	p	f, ph
Surds	k	k	κ	c, q	h, g (f)	h, g, k
	t	t	τ	t	th, d	d
	p	p	π	p	f, v	f, v

the method of analysing, for a child seven or eight years old, the Latin words *tectum*, *frango* and *calidus*, and their philological equivalents in French, German, and English.

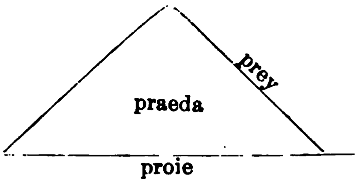
[1]T [2]E [3]C [5]T [6]UM	[1] Dental letters all correspond.
[1]T [2]O [4]I [5]T [6]( )	[2] Vowel " " "
[1]D [2]E [3]CK ( ) [6]( )	[3] Guttural " " " and duplicate.
[1]D [2]E [3]CK ( ) [6]EN	[4] Guttural broken down into i.
	[5] Dental letters correspond.
	[6] Terminations omitted for *toit* and *deck*.

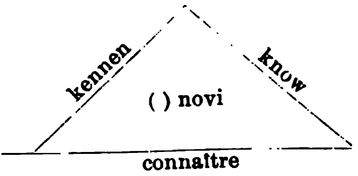
[1]F [2]R [3]A [4]N [6]G [7]O	[1] Labial letters all correspond.
[1]F [2]R [3]I [4]N [6]G [7]E	[2] Liquid " " "
[1]B [2]R [3]EA [4]( ) [6]K [7]( )	[3] Vowel " " " or duplicate.
[1]F [2]R [3]A [4]( ) [6]G [7]I [7]LE	[4] Liquid " " " ; omitted for *break*, *fragile* and *brechen*.
[1]B [2]R [3]E [4]( ) [6]CH [7]E [7]N	[6] Guttural letters correspond.
	[7] Terminations omitted for *break*.

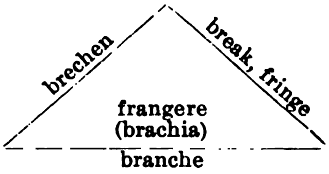
[1]C [2]A [3]L [4]I [5]D [6]U S	[1] Gutturals correspond.
[1]CH [2]A [3]U [4]( ) [5]D [6]( )	[2] Vowels correspond.
[1]H [2]EA [3]( ) [4]( ) [5]T [6]( )	[3] Liquid broken down into u, or omitted.
[1]H [2]EI [3]( ) [4]( ) [5]SS [6]( )	[4] Vowel omitted in others.
	[5] Dentals correspond.
	[6] Termination omitted for *chaud*, *heat* and *heiss*.

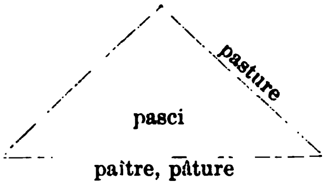
Thence the step is easy to the recognition of the equivalent letters in the corresponding words of different European languages. Practically, he thus learns half a dozen words in the time usually occupied in learning one ; learns them with six time the vividness, and is six times less likely to forget them. Philosophically is laid the basis for the conception of central unities diversified by superficial differences, which is one of the fundamental conceptions of both philosophy and science.

In the *Teacher* for June, 1888, I have described a little device for the comparison of verbal roots, which I have called " language triangles." The child draws one side of a triangle ; in the centre of this he writes the Latin word. Under the base line, and thus parallel to the Latin, he writes the French word, when it really has the same root; and on the two other sides, opposing each other, he writes the German and English. The words so taken must always have the same root. If in either of the four languages the object or idea is expressed by a different root, the space for that language is left blank on the triangle.[1] In addition to those published in the *Teacher*, I subjoin some others.









At a later period of study, and when enough grammar has been acquired to render possible the correct construction of sentences, the comparative study of words may be pursued in a somewhat different fashion. A Latin word may be selected from a sentence in which it has been read, and introduced into a new sentence, to be devised by the child. Then sentences in English, French and German, later in Greek, must be similarly devised so as to introduce the cognate meanings of the same root, with their expressive shades of difference. The system simply extends the method, already in current use, for studying English synonyms. A few examples may serve to illustrate the advantage of this device.

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[1] See *Teacher* for June, 1888. New York.

Multas virorum clarorum statuas in templo *posuit* dux.
Je sais *positivement* qu'il va pleuvoir.
We *postponed* going to the picnic.

Oppidum obsessum *victus* satis habuit.
There was an old woman, and what do you think,.
She lived upon nothing but *victuals* and drink.

Puella regi se *tradidit*.
The boys *traded* marbles with the girls.
Le *traître* a *trahi* le roi.

By such methods, the child, at the end of a year, may be expected to learn from three hundred to five hundred Latin words, and their cognates in French, English, and German. It is not to be presumed that these will be learned beyond the possibility of forgetfulness; but at least the first impression of them will have been made upon the brain. At the same time a beginning can have been made in reading French, whose resemblance of construction (though not of verbal structure) to English makes it easier to read than Latin. A book interesting to the child must be selected, and the reading conducted exactly as it would be in English: the words spelled by the sound of the syllables, the meaning of each word told, and thus the phrase slowly interpreted. When the interpretation is once complete, the child must read the phrase over and over again, until it can be both understood and enunciated fluently; for until the fluency is attained—and it always can be by sufficient repetition—the phrase does not represent language to the child. The attainment of fluency in the reproduction of the impressions made by the written phrase is not only important in itself, but it serves as a type of complete knowledge in any subject. Any kind of knowledge is only thoroughly grasped and digested when all parts of the brain tissue impressed by it vibrate easily and harmoniously upon its suggestion. In reading words, so long as there is stumbling in enunciation, the knowledge has not risen to the rank of an acquired language, the isolated words have not fused into expressive speech.

It is to facilitate fluency, that for at least two years the teacher must supply the place of the dictionary, and tell the child the meaning of each new word. At most, when a word once learned has been forgotten, he may be led to refer to the previous phrase, and to recall the meaning from the context. It is indeed always desirable that the meaning of new words be divined as much as possible from the context. It is wrong to condemn this as reprehensible "guessing" and fatal to accuracy and thoroughness. The act implies an effort of the mind to revive faint reminiscences and to detect faint associations of ideas; it is the very act involved in scientific research after new truths. The teacher must of course be on hand to test the accuracy of the guess, and correct it if wrong or flippant.

The child should not at this period be left to himself at all. Argument as to what is intellectually possible for a child, must not assume that he is to be thrown upon his own resources to interpret the French or Latin page. Such independent work comes later. But the business at first is *not* to train the mind in self-reliance, but to saturate the brain with impressions, and to habituate the ear to a new form of speech. This must be done under guidance, as clay is moulded by the guidance of the sculptor's hand.

With such guidance, Latin—its construction somewhat modified—may be read with but little more difficulty than the French or German. It is essential that the child become accustomed from the beginning to at least the easier peculiarities of Latin construction. It is pitiful to see scholars, after many months, even two or three years of study, still stumbling over Latin sentences, in the attempt to read them in the English order, to turn them, as it is said, "into good English." Now in order to penetrate fully into the spirit of a language, it is necessary at the moment of enunciating it, to banish all recollection of any other language from the mind. It must *not* be translated, or the habit of translation must cease as soon as possible. Then only is it evident that the mind places its different groups of verbal signs on the same footing. One great value to be derived from a fluent acquaintance with Latin and Greek is, that in passing into the unfamiliar construction, the mind passes into a separate consciousness; and by so much enlarges the range of its own experience. This cannot be done to the same extent by means of French, German, or Italian language, because their construction too closely resembles our own.

After two years' study of words, and when by repeated practice, some empirical reading knowledge of French and Latin has been obtained, the child may enter upon the second part of language, the study of *grammar*.

The fundamental peculiarity of grammar has been pointed out. It is concerned with the relations of words and ideas, quite separated from their sensory origins; concerned with processes that take place exclusively in the concept centres of the brain. These are called into function to a much greater extent than is the case even in regard to words, which indeed transcend sense impressions, but not to the extent to which the conception of verbal relations does.

It is perfectly absurd to make a child study grammar until its mind has been well stored with impressions of words. And, on the other hand, it is equally absurd, and a great waste both of time and of fitting opportunity, to defer the study of words until the mind has become ripe for the study of grammar.

Why French grammar should ever be learned before Latin, I have never been able to understand, yet I know it is often done. A large part of its subject matter consists of idioms and conventions, whose reason lies in the historical development of the language, and not in logic. Now a child is capable of logic, long before it is really capable of history. To teach French grammar before Latin, is to accustom the child to place accident before necessity, and convention before truth; a most fatal habit of mind. The two grammars should be studied simultaneously.

In considering grammatical inflections, the child learns to develop an idea whose germ had been previously acquired, namely, that the essence of the word lies in its root, and that the termination is a varying modality. But to fully appreciate this fact, the child must be led to discover the inflections and their groupings for himself, and not confronted at the outset with lists of declensions and conjugations to be learned by heart. This universal practice is, from a psychological point of view, simply barbarous. By a scientific method the child should be led to deduce the inflections from his own observations of the facts of the text. Reading the same word in many different connections, and being obliged, by the context, to translate it differently each time, the child can be led to notice the different termination which corresponds to each translation. From these various observations he can gradually build up for himself, of course under guidance, a complete scheme of the five declensions. Much more time is thus consumed than in the ordinary method of learning these declensions by heart. But, on the other hand, the child repeats the process by which the grammar was originally constructed, and what is still more important, he becomes acquainted with the method which is typical for all scientific study; he collates scattered facts, brings them together, observes their relations, and establishes their law.

The same method applies to the more difficult study of the verb. But here three degrees of generalization are to be observed, that of person, of tense, and of mood. The first distinction is the most general and the most easily appreciated. The extreme regularity of the person terminations in the Latin verb makes them an easy subject for drill. After they have been discovered and established, the distinctions of tense may be similarly dealt with; first, in their broad distinctions of past, present, or future time; later, in the subdivisions of past and future time, that for a long time must seem to the child unnecessarily subtle. Even more subtle are the modifications of assertion implied in moods. I do not think the distinction of indicative, subjunctive, and infinitive mood can really be made intelligible to a child under eleven or twelve years of age, if it can then. But, nevertheless, these moods can be studied descriptively at eight or nine, when they are not explained, but merely characterized by the English auxiliary words used with them, *may*, *might*, *to*, etc.

The inflections of nouns and verbs furnish the child with conceptions of scientific classification at a time when, as already pointed out, these cannot be obtained from physical science. They furnish types of more abstract classification than is afforded by study of word roots, for inflections represent modifications of roots corresponding to modifications of the mind perceiving them. The mind does more than perceive—it handles these roots; it freely manipulates for its own purposes what has hitherto been presented to the child in a purely objective aspect. When a child learns a language on its subjective side first—learns by habit to speak it and use it as a tool, he loses the immense impression obtainable when words have first been studied objectively, as classes of things having a real and independent existence; and the mind is afterwards seen to establish a free dominion over these same things, moulding them to its own purposes, yet leaving their essential nature undisturbed. Here is a splendid type of the action of the human mind in nature, whose details once conquered, may also be inflected to express human meanings.

When the inflections have once been learned, the child must change his mode of reading. He must no longer be told the meaning of words, nor allowed to divine either the root meaning or the mode from the context, but he must infer the precise interpretation of each word and of the entire sentence from these inflected terminations.

It is generally recognized that this act of inference or reasoning is an important mental exercise. Indeed, teachers are rather liable to err on the side of thinking that this is the only kind of mental discipline, and that it is, moreover, the chief value of learning Latin. Neither assertion is true, but the value of the inference is nevertheless great. In it the fact observed, as for instance the termination of the genitive case, is first associated with previous impressions of other similar terminations and the similarity recognized. Then associated circumstances of these previous impressions are revived in memory, as the fact that the termination belongs to such a declension, and is translated by the word "of" in English. The association of these circumstances is transferred to the new impression which has been placed in the same class, and the word therefore interpreted as the others had been.

Acts of inference always imply a similar revival of past impressions, principal and accessory, and their fusion with the impression newly received. They powerfully exercise the mind because they fuse scattered excitations or vibrations into energetic unity. The inferences demanded of the child in translating Latin are simply the type of mental acts that are to be demanded of him all his life, and constitute an excellent preparation for these. The logical value of French and German is so much less, because precise knowledge of construction and inflection is unnecessary to the interpretation, and the general similarity to English renders much narrower the space traversed by the mind to reach the point of view of the foreign consciousness.

All grammatical subjects must be studied on the principles laid down for study of the inflections. The laws, as far as possible, must be deduced from observation of the facts, and not announced categorically, with the facts adduced in illustration. Grammar must be carefully kept subordinate to language considered as a means of expression and communication.

I often think that the feeling for Latin literature is as much injured by excessive drill in parsing, as the literary appreciation of Milton was impaired by the old-fashioned drill in *Paradise Lost.* The study of grammar as a complete and highly abstract science properly belongs only to ripe minds—at earliest, to the period of adolescence.

For children under fifteen, only just so much grammar should be required as is essential to the accurate interpretation of what is read, and to the power of approximate accuracy in writing.

For young children, the selection of grammatical subjects in the order of their real comprehensibility to the growing mind is a delicate, but most interesting task. Two principles should guide the selection. First, that ideas are easy for the child in the degree to which they approach or involve sense perceptions or concrete conceptions, and are difficult according as they recede from these and become generalized. And second, that grammatical laws and rules are impressive in proportion as they seem necessary; and unimpressive, therefore difficult to remember, according as they relate to what seems unimportant, that is, to whatever is unessential to the interpretation of the sentence. Hence the parts of speech which modify the noun and verb are much more difficult to learn about than the noun and verb themselves; and it is illogical to place the study of the adjectives, and especially the study of their comparisons, before the study of the verb. Similarly for adverbs, conjunctions and prepositions, and for all devices for linking words together, and for which the child does not feel the necessity. Similarly for the relations of the parts of a sentence to each other, the discussion of subject and object, the management of the infinitive mood in its relations to the moods of other verbs and to the accusative case; similarly with a host of other subjects that will readily suggest themselves to an experienced teacher, if examined by the test of the principles above stated. A child can become cognizant of a great many grammatical facts at an epoch when it would only be bewildered by the abstract law of these facts. It is easy to learn the fact that if a person or thing is said to be doing anything, the name of this person is put in the nominative case; and this may be intelligible in Latin, when it is quite unintelligible in English. But, at the very same time, the child may be utterly bewildered by the statement, "The subject of the verb is in the nominative case."

Again, it is easy to explain the relations of the subject to an active verb, when it is still very difficult to explain the passive verb or voice. I have noticed that children have the strongest tendency to put the subject of the passive voice in the accusative, because they declare (and with logic) that "something is being *done* to the person." And I think it is hopeless to demonstrate that the terrible verb "to be" is a verb at all. The fact can only be learned empirically, and all explanation of it sedulously avoided. The child confounds this verb with an adjective, and in doing so, merely reverts instinctively to the fundamental conception of the predicate, out of which the verb and adjective have diversely sprung. On the other hand, the picturesque expression of "strong verbs," applied to the famous eleven irregular verbs in Latin, can be easily appreciated by the child, as indicating words worn into irregularities by constant use.

In all study of grammar under the age of twelve this rule should dominate: let nothing be learned but what is essential to the interpretation and manipulation of the language, and defer philosophical grammar to a ripe stage of mental development. The energy often wasted upon premature study of grammar is much more profitably occupied in acquiring fluency in language.

The slow, deliberate, and thorough accumulation and manipulation of verbal impressions enriches the brain. But it is the rapid and instinctive manipulation of such impressions that renders the mind agile and flexible, because it accustoms the brain to the rapid and multiple propagation of excitations, and their varied combinations into secondary excitations.

So far nothing has been said about learning Greek. I think that this should be begun gradually, between the ages of ten and twelve, at first merely by learning proper names and the words cognate to the Latin roots, as these are successively studied. By the age of twelve, a sufficient fluency in the capacity of reading and writing French should have been acquired to justify dropping its study for a while, and substituting the systematic study of Greek, this to be pursued most strenuously during the next four years.

The general construction of a language exhibits on a still larger scale than does its elementary grammar, a process of cerebral synthesis in which the "association" fibres of the brain are involved, those namely which connect separate convolutions with each other. Every special form of language construction depends upon a special grouping, not merely of different areas of cells, but of different convolutions, of distinct territories often widely separated. We may compare these different regions to groups of battery cells, standing on different tables in a laboratory, and labelled A, B, C, D, etc. These groups may be brought into a circuit in various ways according to the order of their connection with one another. Thus we may have,

$A + B + C + D$, or
$A + C + B + D$, or
$A + D + C + B$, or
$B + C + D + A$, etc.,

the variety depending on the laws of permutation.

The different permutations correspond to the different modes in which separate brain regions may be brought into connection with each other, in the general synthesis of cerebral activity that effects the expression of speech according to the construction of a special language.

When a person, habituated to one form of construction, learns to understand fluently, to think, and still more to speak under another form, the functional grouping of these brain regions must be changed. Though the anatomical architecture of the brain remain the same, its functional relationships are rendered different. This change, like all changes for nervous tissues, constitutes an immense stimulus and excitation, proportioned to the extent of the change. To consciousness, the mind seems to have traversed a certain space to place itself at the new point of view. The physical basis of this consciousness is the space occupied by the nerve fibres of the brain, which propagate vibrations from one convolution to another. When an English-speaking person projects his consciousness into the form of language construction peculiar either to Latin or Greek, he seems to traverse a much wider space than if he simply pass from English to French, or even to German. The re-arrangement of direction for the intra-cerebral propagation of vibrations or excitations, must therefore be much more extensive for the ancient languages than for the modern. Hence the mental development, or cerebral stimulus derived, must be much greater.

The special values of the study of Latin over the modern languages may now, in answer to our question, be categorically stated.

1. No European language, and no European history or philosophy, apart perhaps from the Sclavonic and Scandinavian groups, can be understood without knowledge of Latin.

2. Least of all can English language, philosophy, or history be understood, since the language is simply a combination of Anglo-Saxon and Latin, in which Latin considerably predominates, and Rome is indelibly impressed upon English history, thought, and institutions.

3. In the study of words, which should initiate the child into the study of language, the Latin roots are best fitted for beginning, on account of their familiarity, conspicuousness, simplicity, and ready manipulation.

4. The Latin grammar is the most perfect grammar of Europe, and should alone be used to teach grammatical principles, selected in the order of their natural comprehensibility for the developing mind.

5. The construction of the Latin language as a whole compels the translation of the modern mind into a form of consciousness sufficiently remote from its own to necessitate a great change in the general synthesis of cerebral activity. The same is true of Greek. The change constitutes a powerful mental exercise and brain stimulant.

To obtain the full value of the study of Latin and Greek upon the development of the brain, must be applied the principles that are now generally, though half consciously, invoked in the acquisition of the mother tongue and of modern languages, namely, the synthetical impressions of the language as a whole must be copiously stamped on the brain before the pupil is called upon to analyze the language.

This is to be done by means of *much* and *rapid* reading. Roger Ascham tells us that Queen Elizabeth became a good Greek scholar by every year reading entirely through the works of Demosthenes and of Isocrates. The reading must be on a subject interesting to the child; hence it is scarcely possible that it be directed to classical authors usually chosen for a school curriculum. It is the fashion among some teachers to denounce "readers of manufactured Latin," and declare "that the sooner a boy can draw his Latin from the living spring of a classic author, the better."[1] This principle may or not be correct from the point of view of the Latin scholar, but from the standpoint of the physiologist and psychologist it is certainly absurd. We do not forbid English children to read English until they are capable of understanding Milton; or French children from reading French so long as they fail to understand Jomini's Art of War. It seems improbable that Roman children were ever schooled upon Caesar's Commentaries. It would be a poor commentary upon the results of the Latin scholarship of so many centuries, to assert that there are now no scholars capable of writing Latin in a way that should gradually initiate young children into the difficulties of its construction, while accustoming them to look upon Latin as upon any other languages, as a medium for communicating interesting ideas, and not merely as gymnastic exercise for the intellect, concerned with ideas to which the child must be indifferent.

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[1] Six Weeks' Preparation for Reading Caesar. Note to teachers on first page.

An immense number of Latin idoms can become familiar to a child in the same way as French idioms do, by the process of repeated observation of them in the course of reading, and this at a time when the abstract, the scientific statement, or law of those idioms could not really be grasped. Familiarity with the fact should logically precede analysis of the fact. Reversing this process, as is usually done, may make grammarians ; but, unless the study is prolonged many more years than is usually practicable, it does not enable the student to read the language. It is very rare to find that a boy or girl who has begun to study Latin at twelve can read Latin fluently at sixteen, though far more time is given to the study in these four years than should be the case, for they are too precious and too much needed for other things. If during the four years preceding twelve, familiarity with the phenomena of Latin had been acquired by frequent repetition, the subsequent scientific analysis of these phenomena, *i. e.* the grammatical study of the language, would be ten times as fruitful of result.

The development of our subject has insensibly furnished the answer to another of the questions started at the beginning of this essay. It is necessary to maintain a just proportion between the study of languages and the other studies of a general curriculum. The effect on mental development and training is to be obtained, if at all, by the age of fourteen, fifteen or sixteen. By this time the pupil requires the broader and more robust discipline of other knowledge, pursued with the thoroughness of scientific method which will then be practicable. It is undesirable to continue the systematic study of languages at this time; they should be dropped altogether, although the habit of reading in all may be most profitably kept up, and other subjects, especially history, studied through their medium.

All that has been here said on the physiological value of the study of language applies to the developing mind—to the stage of development at which signs are being coupled with things, and the "mental couple" raised to the concept centre, and accepted as a unity in consciousness. For the adult mind, accustomed to the use of signs, the acquisition of a foreign language can have no such educational significance. It is true that an adult who has had no training in language, finds such difficulty in undertaking the study of anything else, that he is best advised to acquire a language, especially some knowledge of Latin, before attempting any other study, especially that of medicine. But he cannot derive the same relative benefit from learning the language then as if he had learned it as a child. Moreover, in learning the language, the time is relatively wasted that might be more appropriately spent in learning to grasp larger and more complex groups of facts and ideas than are presented in any but the really philological study of language.

Hence, one great reason for teaching children a reading acquaintance with four or five languages between the ages of eight and fourteen, is that by the latter age they may really know these languages, and then begin to study something else more difficult, or of more immediate practical utility.

Nevertheless, some study of language must always accompany all other studies. Language which alone perfectly expresses all internal thought, also mirrors all external things as they have ever impressed the mind of man. Language, speech, is thus truly the Logos, the intermediary between the soul and the world. It is at once the thought made flesh and flesh sublimated into thought.

But advanced philological study should be regarded as distinctly a specialty, as is the advanced study of philosophy, or of chemistry, or physics, or physiology, or any other science. That a youth must have, or pretend to have, a perfect knowledge of Latin and Greek before he attempt to acquire even a smattering acquaintance with the world around him, is certainly a traditional superstition. But by the method of language study which has been here advocated, the student may really experience the discipline conferred by language training, may enjoy the immense practical advantage of admission to all European literatures, and yet secure time for a correlatively liberal education in other directions, equally important.

PSYCHOLOGICAL LITERATURE.

I.—THE NERVOUS SYSTEM.

Ueber Entwickelung des Hirnmantels in der Thierreihe. Dr. Edinger. XIII Wanderversammlung südwestdeutscher Neurologen und Irrenäzte zu Freiburg i. Br., Juni, 1888.

Abstract of proceedings in Neurolog. Centralbl. 1888, No. 14, by Dr. L. Laquer. Dr. Edinger showed preparations illustrating the development of the forebrain in the animal series. The brain mantel only gradually reaches the high development which it attains in the mammals, but there is not an unbroken series from the lowest to the higher forms. The purely epithelial brain mantel of the bony fish, the cyclostomes, and selachians, was demonstrated. Between these and the simple amphibian brain there are no intermediate forms. The fundamental form of the amphibian brain is to be found among the reptiles, but among the reptiles there appears, with the beginnings of the cortex, the earliest form of the brain from which that of the birds and mammals has been developed. In reptiles first appears the Ammonsformation and the associated Fornix. While the mantel undergoes all these changes, the position and structure of the ganglia of the trunk remain in general the same through the entire series, decreasing, however, in relative importance with the increase in the mantel. Commissural fibres and fibres connecting parts of the forebrain with other regions, are found in all cases.

Untersuchungen über die vergleichende Anatomie des Gehirns. 1. *Das Vorderhirn.* L. Edinger. (Abh. d. Senckenbergischen naturf. Gesellsch. 1888, p. 91 bis 119, 4 Tafeln.) Abstracted by Obersteiner in the Centralbl. f. Physiol. No. 12, 1888.

In the bony fish the brain mantle covers the basal ganglia in such a manner as to be usually overlooked. A cortex with nerve cells is wanting in all fish and amphibia, and in the reptiles the first form of the cortex with ganglia appears. In the reptiles, too, appear the first fibres of a corona radiata. In birds the basal ganglia are developed to an extent not found in any other group, the cortex remaining but little developed and first reaching its full significance in the mammals. From the basal ganglia (the nucleus caudatus and putamen) arises the basal frontal tract (basale Vorderhirnbündel) which runs in part to the optic thalamus, and in part to portions further caudad.

Anatomy of the Brain and Spinal Cord. J. Ryland Whitaker. Edinburgh: E. and S. Livingstone, 1887. 8vo, pp. 135.

The title of a book like the one in question does not at the present day give a clear notion of what it may contain. Some years back, the gross anatomy was almost exclusively meant by the term anatomy of the central nervous system, and to this gross anatomy there was now and then added a little on the tracts in the cord, and the description of one or two frontal sections of the brain and cord. Beyond this, the descriptions applied mainly to the surface of the organs, though not uncommonly something on the development of the brain was appended. Such a presentation of the subject answered the purpose very well when the anatomy of the central nervous system was but little developed. To-day, however, it has left this earlier condition far behind. The student wants to know, and must know, the finer anatomy of these organs, and the gross anatomy should be presented only in so far as the parts described and the names given are found to be really significant in the light of existing facts. For example, a clear idea of the arrangement of the parts about the lateral ventricles and the interbrain cannot be gotten unless the development of the brain is most carefully considered, and the changes from the primary to the secondary conditions are traced in ample detail. It is on such a knowledge only that a good understanding of the finer anatomy of this region can rest, and the same is essentially true for all the other regions of the nervous centres. Supposing these views to be correct, then a modern discussion of the anatomy of the central nervous system should contain somewhere in it a careful account of the embryology of the brain and cord, as a necessary corner-stone.

Whitaker's book does not recognize this aspect of the case, for its discussion of the embryology is very casual, and it goes along as though there was very little outside of its covers, although the allusions to the finer anatomy are scanty and often antiquated. Looked at in another way, however, it is a handy volume, containing rather more than one gets in the brain and cord chapters in the anatomical text-books, and the order of presentation is good. There are numerous plates, some of them original, the one showing the distribution of the tracts of the cord being specially useful. A very good feature, too, is the tabular arrangement, showing in a general way the representation of the parts seen in one cross-section in the section at another level. As may therefore be seen, the book will be useful where the gross anatomy of the brain and cord is to be studied, but for purposes beyond this its value is limited.

Annual of the Universal Medical Sciences. Edited by C. E. SAJOUS, M. D., and seventy associate editors. Issue of 1888, 5 volumes. F. A. Davis, Philadelphia and London.

An annual review of the progress of the medical sciences that fills five volumes, more than 2500 pages, and is liberally illustrated, certainly calls for remark. According to the preface, the interest of the Annual is in clinical data, and it is designed to be specially useful to the medical practitioner. Since through the chief editor it is intimately connected with the Jefferson Medical College in Philadelphia, one is not surprised to see that a large number of the articles are from men residing in that city, more than half the number of associate editors being Philadelphians. The articles are grouped under some seventy heads. They are not arranged always in the order which might be anticipated, but this is explained by the very limited time in which the work was put through the press. The work is more than a year-book in its plan, for many of the articles are really short treatises, with a few references to the literature of 1887 put in at the appropriate points of the discussion. This gives it the value, in many cases, of a reference handbook. Such a work must of necessity be selective. It cannot review everything, and in this case we find about 2000 titles are referred to, the majority of these being for the year 1887, although they are not all within that limit.

The index of authors for 1887 as it appears in the Index Medicus includes something over 12,000 names. It is plain, therefore, that but a fraction of the literature is discussed, but at the same time it is only fair to suppose that the fraction in question has been selected as being that of greatest value.

One noticeable feature is the large corps of correspondents situated in all parts of the world, many of them in remote regions, who are expected to report on medical matters in their neighborhood. Such a plan, if carried out, must naturally lead to the accumulation of valuable clinical data.

In connection with the Annual there is also published, under the same auspices, a small quarterly journal bearing the title of The Satellite [of the Annual, etc.], the aim of which is to review the most important articles in the medical press at large. The first number of this journal appeared more than half a year before the Annual itself.

The impression that these two associated publications leave is not one of satisfaction. We are not speaking now of the individual communications, but of the book as a whole, indicating as it must a tendency.

For those who are exceedingly busy with such occupations as do not permit much time for reading, or for those who are remote from libraries, the Annual fills a gap, and as the numbers in both these classes are numerous, it may expect to be well received. The student and investigator will probably continue, on the other hand, to use the Index Medicus for the literature, the Centralblätter for the analysis of the current literature, and the various special year-books in which the articles reported are separately analysed, for getting a general view of advances made in former years.

The best articles in the Annual cannot fail to be both instructive and interesting to the reader, as giving the connected views of good authorities, but they furnish him in their present form with very little first-hand material on which he can form judgments for himself. This point of view would be admitted, we are sure, by no one more readily than by the writers of the articles themselves. In our opinion, then, the Annual is but a slight addition to the force of working books on which the student depends. It is with interest that one awaits the developement of the issue for 1889, for so much of the present volumes is necessarily standard matter elaborated in order to show the precise relations of the advances of the year, that it is not easy to see how it can be repeated in a subsequent issue without losing freshness, and if it is not repeated in some measure, the narrative style in which the articles are now written will be maintained only with difficulty.

Turning now to some of the articles which relate to the nervous system, the first volume opens with the discussion of "The Diseases of the Brain and Spinal Cord," by Dr. E. C. Seguin. In treating here of the advances made in the localization of function in the human brain, the author thinks that comparative physiology can claim but very little credit for the present results, and that the same method which led Broca and Hughlings Jackson to locate their respective centres would have given us all the information which we now possess about centres, quite independent of the fact whether animals were or were not studied. That the details of the human brain are to be made out from the study of the human brain alone is a point that will probably be admitted on all hands; but that we should be where we are now in cerebral anatomy and surgery, without the study of the brain in lower forms, is by no means to be readily admitted when we compare the advances in the period which elapsed between Broca's observation and the fundamental experiment of Fritsch and Hitzig, with those in the same number of years following the latter.

The second volume opens with an account of the surgery of the brain and nerves, by Dr. N. Senn, in which the modern surgical methods as applied to the brain are detailed.

In the fifth volume Dr. Spitzka gives fifteen pages to the anatomy of the brain. A number of the papers which form the basis of this article have been given in abstract in this journal during the past year, but others have not been mentioned. In discussing the paper of Adamkiewicz on the circulation in the ganglion cell, and the observation of Fritsch that blood-vessels are found within the protoplasm of the giant cells in Lophius piscatorius, Spitzka seems a little hasty in saying that observations have been made which entirely dispose of the old view that the ganglionic element is the equivalent of a simple cell. In the first place, the observations of Fritsch have little in common with those of Adamkiewicz, and it is hardly fair to class the two together; and in the second place, if our notion of the simple cell is to be disturbed by finding some other structure in its protoplasm, it should long ago have been upset by finding intracellular nerve terminations and the nephridia, etc., which are intracellular in the invertebrates. Most interesting are Spitzka's own observations on the cetacean brain, in which all the parts connected with the auditory nerve are found in such a hypertrophic state on so comparatively simple a background, that it affords not only much evidence in favor of views derived from the study of animals less suited to show these points, but stands as one of the most striking contributions of the comparative method to the finer anatomy of the brain.

At the meeting of the Congress of American Physicians and Surgeons held at Washington, September 18–20, 1888, neurological matters had much attention even outside of the discussions in several societies. On the evening of September 19th there was a general discussion of localization in the brain with special reference to brain surgery. The papers of the evening were unusually satisfactory, but any special mention of them must be omitted here, because of their rather practical character. The general sentiment appeared to be that surgical interference with the brain was now attended with comparatively so little danger from the side of the operation that there might be too much surgery, and the more conservative speakers added a word of caution on this head. The remarks of Mr. Victor Horsley on an investigation which he had made in connection with Mr. Gotch, on the stimulation of the cerebral cortex in monkeys, was an experimental contribution which can perhaps be summarized.

In an attack of Jacksonian epilepsy there is first a tonic followed by a number of clonic spasms. It is known that the initial discharge takes place from the cortex, and therefore the tonic spasm is of cortical origin, but the question still remained as to the origin of the clonic spasms. These might arise either by separate discharges from the cortex, or by a rhythmical discharge of the spinal centres consequent upon a single stimulus from the cortex. Relying on the fact that each separate impulse as it passes along a nerve gives rise to a negative variation of the resting nerve current, then, if it were possible to tap the pyramidal tract above the spinal centre while the cortex was being stimulated, and examine the negative variations, it would be seen whether a series of impulses were coming from the cortex at the proper rate to account for the clonic muscular contractions. In the first place, the authors succeeded in so operating on the spinal cord in the monkey that it could be kept alive and suitable for study for nearly half an hour. With non-polarizable electrodes, they then led off the resting nerve current from the cut and longitudinal surfaces of the pyramidal tract to a capillary electrometer—a capillary tube containing a column of mercury, the height of which varies with slight variations in the electrical tension. So delicate is this instrument that it responds quite satisfactorily to variation in the nerve current by a change in the position of the mercury in the tube. The amount of this change is magnified by viewing it through a microscope. In these experiments the oscillations of the mercury were recorded photographically.

Stimulation of the cortex in the leg area gave rise to a prolonged negative variation corresponding to the tonic period; then, on the removal of the electrodes from the cortex, there followed a series of variations corresponding perfectly to the clonic period of the muscular disturbance. The origin of the clonic contractions is therefore cortical. That the result is not due to a diffused disturbance in the cord is shown by the fact that if the electrometer be undisturbed and the arm centre in the cortex be stimulated, there is no evidence of any electrical variation. An attempt to tap the motor nerve roots and test the negative variations there led to no results, the disturbance being too slight to affect the electrometer. The results in this case are highly interesting, but hardly less interesting are the several very refined methods of operation and observation by which these results were obtained.

A Comparison of the Latency Periods of the Ocular Muscles on Excitation of the Frontal and Occipito-temporal Regions of the Brain. E. A. SCHAEFER. Received February 13, 1888. Proc. Roy. Soc. Vol. 43.

The very condensed statement which the author makes of these experiments cannot be further abstracted without some important omissions. We give, however, his main points. The conjugate deviation of the eyes to the opposite side is produced by the excitation of entirely different regions of the cerebral cortex. The parts which, when electrically excited, produce this movement, are: 1, an area included in the motor or psychomotor zone of the authors; 2, the sup. temporal gyrus; 3, the upper end of the middle temporal gyrus; 4, the post. limb of the angular gyrus; 5, the whole cortex of the occipital lobe, including its mesial and under surfaces; 6, the quadrate lobule. Of these parts, the frontal area is distinguished by the fact that its excision causes paralysis of the movement. From this fact Ferrier concluded that in the case of the frontal area the excitation was direct, while in all the other cases it was indirect, *i. e.* through subjective sensations. To test this, S. examined the latent period of stimulation of the ocular muscles when excited through the various regions named, and found that it was some hundredths of a second less in the case of the frontal area than for any of the others, thus indicating that in the case of the latter the impulses must pass through at least one more nerve centre than in the case of the former. It was most natural to infer that this other nerve centre would be the frontal area. But that this is not so is indicated by the fact that complete excision of the frontal area on both sides does not abolish the reaction when caused by stimulation of the other portions of the cortex. What the other centre may be is therefore still left doubtful. The work was done on monkeys.

On Electrical Excitation of the Occipital Lobe and Adjacent Parts of the Monkey's Brain. E. A. Schäfer. Rec'd Feb. 13, 1888. Proc. Roy. Soc. Vol. 43.

By stimulating the parts of the cortex named, S. has found that not only were movements of the eyes obtained, but that the direction of these movements bore a relation to the portion of the area stimulated. This is the reverse of Ferrier's results, who got no movement from the occipital cortex, and a refinement of the results of Luciani and Tamburini, who obtained a simple conjugate deviation of the eyes. The regions from which movement of the eyes can be gotten by stimulation of the cortex in and about the occipital lobe are named in the preceding abstract. This area is divided by S., according to his results, into three zones—an upper, middle, and lower, enumerated from above downwards. The parts about the parieto-occipital fiss. form the upper; the inferior zone comprises the whole inferior surface of the lobe and the lowermost parts of the convex and mesial surfaces; while the middle zone lies between these two extremes. An excitation of the superior zone causes movement of the eyes downwards; of the middle zone, a lateral deviation, and of the inferior zone, a movement upwards. It is therefore inferred that the superior zone is connected with the upper lateral portion of the corresponding half of each retina, the middle zone with the middle portion, and the lower zone with the lower portion. S. concludes: "If we imagine the visual areas of the two cerebral hemispheres to be united in the middle line, we may conceive each retina as projected in its normal position over the united area. It will then at once appear that the upper and lower parts of both retinas will fall upon the corresponding parts of the united area, that the outer part of the left retina and the inner part of the right will fall on the outer portion of the left side of the united area, and *vice versa*, and that a vertical line bisecting each retina will fall along the line of union of the two cerebral visual areas. The parts concerned with direct or central vision will therefore correspond with a part of the mesial surface, and each pair of 'identical points' of the retinas will correspond with one and the same spot of the cerebral surface."

Ueber die Entwickelung der Furchen und Windungen des menschlichen Gehirns. J. MINGAZZINI. Moleschott's Untersuchungen, XIII, 6, S. 498. Reviewed in Centralblatt f. Physiol. No. 5, 1888, by Ziehen.

The chief point of this investigation was to determine at what stage of development the variations in the convolutions became apparent, whether they appeared simultaneously on both sides, and other related facts. 42 foetal brains hardened in alcohol and zinc chloride were the material used in the study. M. finds that almost all the variations occur between the seventh and tenth months of foetal life—quite what is to be expected when it is recollected that the main fissures and sulci alone are marked out in the seventh month. Certain sulci are not simultaneously developed on both hemispheres. For example, the fiss. occipit. II appears more often first on the left side; on the other hand the sulci orbitales and supraorbitales first on the right side. The frontal sulci appear with perfect regularity first on the right side. The growth in the length of the sulci is unequal on the two sides, and of the secondary sulci some appear between the seventh and eighth months, others between the seventh and ninth months, while the tertiary sulci appear between the seventh and tenth months.

Differences between male and female show in the development of the gyri from the eighth month on. These consist in the male not only in an absolutely greater cerebral surface, but also in a relatively greater growth of the parts lying in front of the central fissure as compared with those lying behind it.

Ueber die Lymphwege des Gehirns. M. J. ROSSBACH und E. SEHRWALD. (Centralbl. f. d. med. Wissenschaften, 1888, Nos. 25 und 26.) Abstracted in Centralbl. f. Physiol. 1888, No. 12, by Obersteiner.

It has been suggested here and there of late that the stain produced by Golgi's method for bringing out the ganglion cells depended on a deposit of silver or mercury salts in lymph-spaces. The work of these authors goes far to support such a view, and they interpret their results as showing them three sets of lymph-spaces in the brain, those about the vessels, about the nerve cells, and about the glia cells. The relations of these spaces to the perivascular spaces and to one another are such as have been described for the prolongations of the respective sorts of cells.

It should be added in support of the view here taken, that by this same reaction the authors have been able to demonstrate lymph-spaces in many other organs, as the intestine skin, liver, muscle, cartilage, etc.

Etwas über Schädel-Asymmetrie und Stirnnaht. M. O. FRÄNKEL. Nuerolog. Centralbl. No. 15, 1888.

It is certainly still open to discussion how far the development of the brain is associated with that of the skull, and whether it is safe to infer from a deformation of the skull a corresponding variation in the brain. By the younger Italian school, asymmetry of the skull is considered as a degenerative change, and their statistics go to show that it is a marked characteristic of the criminal class. Other authors look upon a moderate amount of asymmetry of the skull as quite normal. As concerns the brain, Broca has remarked that the asymmetry of the convolutions is the special advantage of man and the more highly developed animals, while the convolutions in the primates, negro, and idiots, tend to become more and more symmetrical. Such ideas as these are of course quite out of harmony with those of the Italian school. For the purpose of seeing whether the skulls of the lower animals corresponded with their more symmetrical brain development, the author studied the relations of the frontal suture on many existing and some extinct species, and found all plainly asymmetrical, and some so to a very considerable degree. It appears from this study that brain and skull are not so interdependent, and further, that there may be some reason for considering asymmetry as the rule in the development of animal structures, and that when the Italian school point to the asymmetry of the skull as a characteristic of the criminal class, the abnormality really lies in the excessive development of the difference between the two sides rather than in a departure from perfectly symmetrical growth.

Kraniometrie und Kephalometrie. Vorlesungen gehalten an der Wiener Allgemeinen Poliklinik von M. Benedikt. Mit 36 Holzschnitten, viii und 172 S. Wien und Leipzig, Urban und Schwarzenberg, 1888. Reviewed in Neurolog. Centralblatt, No. 10, 1888, by Sommer.

The author has first to call attention to the relations between the atypical development of the skull and abnormal brain functioning, while the final goal of craniology is from the study of the form of the skull to infer all the laws of its growth. In his own studies he has used an elaborate instrument called an optical kathetometer. From his investigations, he is led to the view that the exterior of all skulls presents a definite number of spherical surfaces, often with very various radii; that these stand in relation to definite portions of the brain, and that between these two there is a fixed relation of growth. To determine the centres for the spheres which these surfaces represent, and to compare the changes that these centres experience with the growth of the individual, etc., are, according to Benedikt, lines of research which would be very profitable, but which he has not followed. Among the special points which he has made out are that in cases of congenital (or early acquired) blindness, there is a noticeable shortening of the interparietal arch; in congenital aphasia, stenokratophy, in deafness, a shortening of the temporal arch; in epilepsy, a deformation of the parietal bones, and in criminal and psychopathic individuals a flattening of the frontal bone. Finally, he discusses the methods for determining the capacity of macerated skulls, and finds no method which is thoroughly satisfactory.

Ueber die Erregbarkeit einzelner Faserbündel im Rückenmark neugeborener Thiere. W. Bechterew, in Kasan. Neurolog. Centralbl. No. 6, 1888.

In attempting to test the function of different bundles of fibres in the spinal cord, the author has hit on the happy idea of using newborn animals. As is well known, only a portion of the bundles of fibres in the cord are medullated at birth. Bechterew assures himself that those which are non-medullated are neither irritable nor conductive, and, further, that by using weak electrical stimuli, the escape of the current, so much talked of, in such observations, is to be little feared. These facts being accepted, the results have much value.

He experimented on puppies without any anaesthetic. In new-born puppies, the lateral and anterior portion of the posterior columns, the so-called root portion of the columnae cuneatae, the fundamental tracts in the anterior and lateral columns, and the direct cerebellar tract, are alone medullated. About five days after birth, the columns of Goll become medullated, and eleven to thirteen days after birth the pyramidal tracts acquire their sheaths.

Stimulating the cut section of the cord in new-born puppies, in the region of the root fibres of the cuneate columns, produces a contraction of the muscles which are innervated by motor nerves arising at that level, much the result one would get from stimulating the posterior nerve roots of the region.

Stimulation of the columns of Goll, five days, later produces reflex contractions of the head, trunk and limbs, as in the adult, but without any indications of pain. It will be seen that the reaction is more diffused in this second case. The stimulation of the antero-lateral fundamental tract in the caudal portion of the cord, the section having been made in the cervical region, produced contractions in the fore and hind limb of the same side, and in the tail, thus indicating the connections of these fibres.

Stimulation of the central end of the cord in the anterior part of the lateral region gave indications of centripetal fibres in this region, probably the tract described by Bechterew and Gowers, while the stimulation of the direct cerebellar tract gave characteristic movements of the head and trunk.

Not only, therefore, are these separate bundles in the cord each excitable, but each has a more or less distinct reaction.

Ueber die centralen Endigungen des N. vagus und über die Zusammensetzung des sogenannten solitären Bündels des verlängerten Marks. W. BECHTEREW. Wjestnik psychiatrii i nevropatologii, 1888, V. 2, Russisch. Reviewed in Neurolog. Centralbl. 1888, No. 10, by P. Rosenbach.

For the study of the terminations of the vagus fibres in the medulla, Bechterew found fœtuses about 28 cm. in length best suited, because at that stage of development it is mainly the nerve roots that are medullated, and these can then be easily followed by Weigert's method. The vagus fibres take several courses within the medulla. A considerable portion goes direct to the vagus nucleus. Another portion crosses the middle line to the N. ambiguus of the other side, while some fibres end in the N. ambiguus of the same side. Finally a portion goes to the Funiculus solitarius of the same side. The fibres entering the funiculus after a time emerge from it, and crossing the middle line, appear to end in a group of cells which lies mesial of the hypoglossus roots, dorsad of the inferior olive, and is longitudinally co-extensive with the hypoglossus nucleus. (This nucleus was described by Misslawski, see Neurolog. Centralbl. 1886, p. 560, and was stated by him to be the most important reflex centre for respiration.) The only other fibres which enter into the F. solitarius belong to the glossopharyngeus, and are stated to rise from a group of small cells which lies mesial and cephalad of the F. gracilis, at the level of the superior pyramidal decussation.

Beiträge zur Kenntnis des Centralnervensystems von Lumbricus. BENEDICT FRIEDLÄNDER. (Berlin.) Zeit. f. wiss. Zool. Bd. 47, pp. 47–83, pl. IX, X, September, 1888.

The author's chief attention was directed to the relationships and not to the ultimate structure of the histological elements. Of the three problematical "neural canals" or "giant fibres" of Leydig, the two lateral ones are shown to be directly continuous with the processes of ganglion cells at the posterior end of the ventral nerve cord, while all three at the anterior part are connected with common nerve fibres. The sheaths of these three structures are composed of connective tissue fibres, and are not comparable to the medullary sheath of vertebrate nerve fibres: the contents is a homogeneous plasma that may be squeezed out in elongated masses. Each lateral "giant fibre" receives the ascending processes of several large bipolar ganglion cells lying ventrally in successive ganglia near the posterior end of the cord, while its most posterior connection is with the similar process of an unipolar ganglion cell. These ascending processes of ganglion cells are connected with one another by transverse processes, and these in turn with the median "giant fibre." Though thus composed of fused cell processes, the "giant fibres" appear quite homogeneous in all the best preparations, and no indication of such complexity of structure as that claimed by Nausen could be obtained, though the author will not deny that such structure may exist. Artificial and deceptive results, due to imperfect means of hardening, are common and difficult to avoid. The "giant fibres" are undoubtedly nervous structures of unknown function; their sheaths may have acquired a secondary importance as aiding in stiffening the ventral nerve cord.

E. A. A.

Histologische Untersuchungen über das Nervensystem von Amphioxus. E. ROHDE. Zool. Anzeiger XI, 190; Vorläufige Mittheilung. Abstract in Centralbl. f. Physiol. No. 10, 1888, by Paneth.

This paper gains much interest from the recent description of colossal nerve fibres in the ventral cord of Lumbricus and other worms. The central nervous system consists, in Amphioxus, of cells which lie near the central canal, and of fibres surrounding them. The supporting substance is formed by the basal prolongations of the ependyma cells. In some cases the prolongations are branched, forming a fine network. The ganglion cells are uni-, bi-, or multipolar, the last being most numerous. Their prolongations form the external nervous substance, which is mainly constituted of fine longitudinally coursing nerve fibres, in which dichotomous divisions are frequent. There are, moreover, a number of very large fibres, definite in both number and position, which arise from large multipolar ganglion cells. These prolongations are of two kinds; all except one lose their size by repeated divisions and form fine longitudinal fibres, while this one passes without any diminution in size, caudad to the other end of the cord. The paired fibres from the anterior end arise from twelve ganglion cells. There is an analogous arrangement of cells and fibres in the posterior portion of the cord, save that the number of cells is fourteen, and that the large fibres coming from them pass cephalad to the anterior end of the cord. In this course they give off fine branches, but undergo little diminution in size, and finally terminate rather abruptly at the head end. (The failure of some of these colossal fibres to diminish in size during their course is a fact that needs further study.)

Functional Nervous Diseases, their Causes and Treatment. Memoir for the Concourse of 1881–1883, Académie royale de médecine de Belgique, with a supplement on the anomalies of refraction and accommodation of the eye and ocular muscles. GEORGE T. STEVENS. New York, D. Appleton & Co., 1887.

The title of this book is quite misleading, for the discussion of functional nervous diseases and treatment is almost exclusively limited to the relations which abnormalities of the eyes and the ocular muscles may hold to them. The author has particularly noticed that the eyes are abnormal in a large number of cases of functional nervous diseases, and further has found it possible to cure and relieve many of them by treating the eyes. He recognizes that unstable nervous systems arè found, that the condition of instability may be hereditarily transmitted, and that the irritation proceeding from disordered eyes may be a stimulus strong enough to produce a functional disturbance in an unstable nervous system, without, perhaps, making it very plain by what he says that any strong stimulus may produce the same result, and that the instances which he presents are to be considered as special examples of this well recognized fact.

Studien über den feineren Bau des Geschmacksorgans. FRIEDRICH HERMANN. Erlangen, 1887. Druck der Universitätsbuchdruckerei von E. Th. Jacob. 8vo, SS. 41.

The first part of this monograph is occupied with an historical review and critical discussion of the results and conclusions reached by various investigators respecting the more intimate structure of the taste-bulbs. The remaining portion contains the results attained by Hermann, who confined himself, almost exclusively, to an examination of the foliate papilla of the rabbit. The supporting cells of the taste-bulbs, he says, are not flat cells, as supposed by some previous observers, but are spindle-shaped cells filled with fluid. They are of two kinds, inner and outer supporting cells. The outer cells, which he designates "pillar cells," and which constitute the true supporting element of the bulb, are pyramid or spindle-shaped cells, having their basal ends divided into a number of fine processes. The cell-body is marked by a distinct network of fine meshes. The nucleus is situated in the lower half of the cell-body, and contains two or three nucleoli. The inner supporting cells, which are fewer in number than the preceding, are cylindrical in form, having enlarged bases which break up into fine processes. The peripheral end of these cells does not bear needle-shaped processes. The nucleus is elliptical and lacks true nucleoli. These cells, Hermann thinks, may be those described by Schwalbe as "staff cells," and supposed by him to be sensory in function. Hermann describes a third kind of supporting cell, flat or conical in shape, and which rests upon the mucous membrane at the base of the bulb. These cells, of which there are from two to four in each bulb, he calls "basal cells of the buds." They are furnished with an oval nucleus, and send out many delicate processes which divide dichotomously, and, by means of the network thus formed, are in connection both with each other and with the stroma of the mucous membrane. In transverse sections through the bulbs, the basal cells are seen to form a protoplasmic net, in which the author sees an analogous formation to the olfactory mucous membrane. In the stroma underlying the bulbs are dense fasciculi of very fine nerve-fibrils, which disappear in the protoplasmic net of the basal cells. Within the bulbs frequent examples of nuclear division are present. Karyokinetic figures were seen most frequently in the basal cells, and very rarely in the "pillar cells." Hermann, from this fact, ascribes to the "basal cells" the rôle of acting as compensating cells for the taste-bulbs. The granular masses of v. Vintschgau he looks upon as degenerate "pillar cells." Respecting the taste-cells, he adds but little to what is already known. The number of these cells, he thinks, has been underestimated, there being, according to his statement, from ten to fifteen nerve-cells in a bulb. Passing from the gustatory pore inwards, he recognizes a second circular opening (within which may be seen the peripheral terminations of the "pillar cells"), for which he suggests the name "inner gustatory pore." F. T.

Untersuchungen über die Papillae Foliatae et Circumvallatae des Kaninchen und Feldhasen. O. Drasch. Abhandl. d. K. S. Gesellsch. d. Wiss., Bd. XXIV, S. 231-252. Mit 8 Tafeln.

In a former memoir (*Sitzb. d. k. Akad. d. Wiss. Wien*, Bd. 88, Abth. III, 1883) Drasch published the results of an investigation of the intimate structure of the foliate papilla of the rabbit and hare. The present paper deals in general with the same subject, and is designed to supplement his earlier treatise on the taste organs in mammals. In the first paper Drasch made the statement, which he has since been able to confirm, that the sensory cells present in the bulbs could not be a criterion for the sum of the taste-fibres of the glosso-pharyngeus nerve. In other words, the number of nerve-fibres into which the glosso-pharyngeus divides, directly below the bulb region of the various taste organs, far exceeds the sum of all the sensory cells in those organs. Beneath the basal membrane of the secondary leaf of the papilla foliata is a plexus formed of medullated nerve-fibres. From this plexus, fibres, corresponding in number to the sum of the sensory cells, go directly to the bulbs. Other fibres, more numerous, pass between the bulbs to the epithelium situated above them. Many fibres, however, terminate in the membranous stroma. Below the bulb region, in the entire width of the leaf, is found a connected stratum of ganglion cells which contribute to the multiplication of the fibres. In addition to the foregoing investigation, Drasch noted the changes produced in a papilla when subjected to various kinds of stimuli. If a normal papilla be pressed upon by a glass rod or stroked with a brush, no secretion of the glands follows; but if a needle or bristle be introduced into a furrow and moved about, secretion takes place. Weak induction shocks applied to the surface of a healthy papilla, or stimulation of the peripheral end of the divided glosso-pharyngeus, cause profuse secretion. Eight days after division of the nerve, the exterior of the papilla does not exhibit any important change visible to the naked eye, but stimulation of the peripheral trunk no longer produces secretion. If, however, the surface of the papilla be exposed to strong induction shocks, the glands continue to secrete for a while longer. By the fifth or sixth week no farther secretion takes place. In the case of a rabbit, investigated six months after division of the glosso-pharyngeus, the divided nerve had united, and the papilla experimented upon appeared to execute its functions quite normally. These experiments, Drasch says, "prove that in general, all gustable substances, when brought upon the taste papillae, or near them, induce secretion of the lingual glands, discharging into the furrows and trenches of the papillae. This secretion is due to reflex action, . . . and is brought about chiefly by means of the intra-epithelial plexus of nerves situated above the bulbs." "The glandular secretion serves for the washing away of dissolved gustable substances, and for continuous cleansing of the papillae. The time that elapses between touching the papilla with a gustable substance, and the subsequent secretion, must be such as to allow the substance in solution to penetrate as far as the bulbs. Yet the hypothesis, that over the entire papilla there are scattered fibres (having a free ending) which are capable of tasting is not inadmissible." F. T.

On the Auditory Labyrinth of Orthagoriscus Mola L. D'Arcy W. Thompson. Anat. Anzeiger, Jahrg. III, 1888, S. 93-96.

Professor Thompson found the auditory labyrinth of Orthagoriscus to differ in some respects from that of all Teleostean fishes. It hangs suspended by webs of delicate connective tissue within a wide space, continuous with the brain-cavity, as in Chimaera. A single vertical pillar of cartilage passes down across this space, within the arc of the horizontal canal. In the membranous labyrinth the following parts are distinguishable: latriculus with sinus superior, recessus utriculi, the three semicircular canals with their ampullae, and the sacculus and lagena. Six nerve-endings are visible, three cristae ampullarum, macula recessus utriculi, maculae sacculi and lagenae. The macula neglecta was wanting, and no trace of the ductus endolymphaticus was seen. No true otoliths are present, but instead the maculae are supplied with many small white otoconia, aggregated together. A few of these have a cubical crystalloidal form, similar to those of Acanthias, but most of them are round or oval, rough on the surface, and concentrically striated within. The proportions of the labyrinth are unusual, the semicircular canals being disproportionately long and the vestibule very small. Orthagoriscus differs from all other fishes except Lophobranchii in the complete conjunction of utriculus and sacculus, that is, in the absence of any distinction of pars superior and inferior. F. T.

On the Fate of the Muscle-plate, and the Development of the Spinal Nerves and Limb Plexuses in Birds and Mammals. A. M. Paterson. Quart. Journ. Micr. Sci., Vol. XXVIII, 1887, pp. 109-129, pls. VII and VIII.

The author concludes from an examination of many sections that the spinal nerves are developed from epiblast throughout their entire length. He has traced the spinal nerves, not only the nerve-roots, but also the trunks and the plexuses, as a centrifugal growth from the spinal cord. The growth of the nerves is both interstitial and terminal. They consist at first of rounded cells, in an active state of proliferation ; in older embryos these become ovoid, and finally fusiform. These fusiform cells, by the alteration of their protoplasm, become converted into nerve-fibres. The development of the nerves in the limbs takes place as follows : The primitive nerve grows out beyond the lower end of the muscle-plate, and reaches the root of the limb. It there spreads out into an irregular series of processes, which pass into the undifferentiated tissue of the limb. These branches, later, arrange themselves in two trunks, one dorsal, the other ventral, which extend still farther into the limb and enclose between them a mass of blastema, from which the cartilaginous basis of the limb is formed. The dorsal and ventral trunks fuse with adjacent dorsal and ventral trunks to form two broad flat bands, from which, still later, the individual nerves as found in the adult are produced. F. T.

II.—EXPERIMENTAL.

Studien über Licht- und Farbenempfindung. A. Eugen Fick. Pflüger's Archiv, Vol. 43, p. 441.

This valuable set of experiments on the limits of the light-sense and the color-sense, which were carried out with the assistance of Fräulein N. Fick, throw doubt on a number of results apparently obtained by Charpentier, and also serve to settle some other points which have been for some time in dispute. A former observation, the correctness of which was denied by Charpentier, is, in the first place, confirmed, namely, that the color of several separate small points is more readily detected than that of one of them. It is then pointed out that the results of Charpentier's quantitative experiments, in regard to the threshold for light and color, show an agreement which would be impossible if they had been conducted with sufficient care, and it is shown that they are in fact erroneous. Any absolute determination of a threshold for the perception of the light and color of a small faint object in an otherwise dark field is proved to be an impossible task, owing to the very great differences of sensitiveness exhibited by different individuals, and by the same individual from day to day, or even from moment to moment, and owing also to the different degree of sensitiveness of different portions of the retina, combined with the impossibility of keeping the eye steady when looking at a dark field. The facts in regard to the latter point have been variously set down,—Aubert and Erdmann, for instance, believing that any apparent difference between the fovea and the adjacent parts of the retina is due to the more rapid fatigue of the latter, and disappears after adaptation has taken place. This Fick shows to be very far from being the case. He secured good fixation by introducing two minute bright points above and below, and looking at a point half way between them. He found that the light-sense and the color-sense present opposite phenomena ; the latter is more acute and the former is less acute at the fovea than at the other portions of the macula lutea. The sensitiveness for light reaches a maximum on the temporal side of the eye, somewhere from 7° to 15° away from the centre; it is here from ten to twenty times as great as at the fovea, which is in all cases the lowest point of the curve. Different individuals furnish curves which differ very much in detail; Fräulein N. had a "fabulous" power of detecting faint lights and colors, but even for her the sensitiveness to light outside of the fovea was two or three times greater than at it. But, for the detection of color, the general shape of the curve is reversed. All colors (if the eye has undergone adaptation by the observer's remaining for fifteen minutes in a dark room) are best perceived at the fovea. Red light has the peculiarity to be seen to be light and to be red at very nearly the same instant, at the fovea. For all other colors much less illumination is necessary to see them than to name them, even at the fovea, and beyond it the difference increases rapidly.

Charpentier stated that the color of a group of points can be named sooner than they can be counted, and exactly four times sooner for all colors. Fick found that so simple a rule is far from holding; a small number of yellow points, for instance, were counted with six times less illumination than was necessary for distinguishing their color. C. L. F.

Ueber das Verhalten der normalen Adaptation. TREITEL. Graefe's Arch. f. Ophth. XXXIII, 2, p. 73.

Aubert found that the sensitiveness of the eye was increased 35-fold by remaining for two hours in a darkened room. Landolt found that with increasing adaptation, the order in which the colors were recognized was green, yellow, red, blue, violet. Treitel, by first blinding the eyes, obtained a difference of visual power of 120-fold. After fatiguing by different colors, the order of recovery for the different colors was as follows:

After Red-fatigue	G, B, Y, R.
" Blue "	Y, R, G, B.
" Green "	$\overset{\frown}{\text{R, B}}$, Y, G.
" Yellow "	R, B, G, Y.

The fatigue must take place in the retina, for it proceeds in the two eyes independently of each other. The fovea is much slower in recovering than the lateral portions of the eye. The coincidence between the time which is required for adaptation to take place and for the visual purple (or rod-purple, as it ought to be called) to become restored, points to a connection between the two processes. This is confirmed by the fact that symptomatic night-blindness occurs with diseases of the eye which attack the pigment-epithelium. Idiopathic night-blindness is always a result of excessive exposure to light; in a poor state of nutrition, among old people, for instance, a slight exposure is sufficient to bring it on.

Die Analyse der Lichtwellen durch das Auge. A. GÖLLER. Du Bois-Reymond's Archiv, 1888, I and II, p. 139.

If natural white light is first polarized, then passed through a thin piece of quartz, and then examined by an analysing prism, it will be found that the quartz has had the effect of rotating the plane of polarization, but by a different amount for the different colors. If the piece of quartz is very thin, the whole spectrum is run through by a rotation of the analyser of less than 180°. This is rotatory polarization, and it is by rotatory polarization that Göller explains the analysis by the eye of ether vibrations into sensations of differently colored lights. Monochromatic light, on entering the eye, passes through the transparent retina, and is reflected back from the pigment-epithelium in a state of plane polarization. The outer members of the cones play the part of the piece of quartz—they shift the plane of polarization by a definite angle. A molecular motion, of much slower period, is then set up in the protoplasm of the inner members, and it is the sensitiveness of the nervous filament to the plane of this motion which constitutes the sensation of color. Two complementary colors are colors which have had their planes of polarization rotated one ninety degrees more than the other, at the same time that the phase of one has become a quarter wave-length behind that of the other, the amplitudes being the same. These conditions, Göller says, would be sufficient to cause their superimposed harmonic motions to produce the motion of circular polarization, and that would be indistinguishable from the motion produced by all the colors of white light acting together. Two vibrations whose planes were at a different angle, would give an elliptic motion of such a kind that the direction of its major axis would give its tint, the excess of the major axis over the minor would give its saturation, and the minor axis would give the amount of white light mixed with it. No two colors other than those described above could give white light, for motion in a circle can only be produced, Göller says, by two rectilinear motions of the same amplitude and at right angles to each other, and thus is explained the fact that most colors when mixed in no matter what proportion cannot be made to produce white light. This exposition seems to contain two grave errors, both depending upon the fact that Göller has apparently overlooked the effect of the period upon the composition of harmonic motions. Two harmonic motions at right angles to each other cannot produce circular motion unless their period is the same; and to suppose that the outer members of the cones, besides the difficult functions already assigned to them, had also the power of equalizing the periods of all rays transmitted by them, would be much too forcible an assumption. Again, it is not true that two simple harmonic motions which are not at right angles to each other cannot produce motion in a circle, if the right difference of phase is chosen to fit their inclination, any more than it is true that the square is the only parallelogram in which a circle can be enclosed. This theory, therefore, which looks rather interesting at first sight, would seem to be utterly untenable. It is possible that some polarization theory might be proposed, with a somewhat different set of assumptions, which would stand examination; the chemical theory is far from being so well established as it is commonly assumed to be. The assumption that there are colorless visual substances, which act after the rod-purple is bleached, and in those places where it never existed, is a purely gratuitous one, and not the slightest reason has been brought forward to support it. C. L. F.

Ueber optische Bewegungsempfindungen. Prof. SIGMUND EXNER. Biologisches Centrablatt, 15 September, 1888.

In this lecture, Prof. Exner brings together so many interesting observations upon a topic to which he has largely contributed, that a somewhat detailed abstract will be desirable.

He begins by introducing into the sensation of motions a distinction between the inference of the motion of a body from the fact of seeing the body in different regions of space at successive intervals, and the immediate perception of motion as a simple elementary sensation. The distinction comes to the front in the observation, originally due to Czermak, that the second-hand of a watch, if observed in indirect vision, seems to move very much more slowly than when directly viewed. But Prof. Exner thinks that in the former case the motion is not perceived, but inferred; in the latter it is directly perceived. To show that it is not necessary for a perception of motion to have the object seen in two successive positions, one need only have the space between the two points, or the interval between the two appearances, so minute that they cannot be distinguished, and yet have a sensation of motion as the result. Under certain conditions two impressions succeeding another with an interval of .045 second are just recognized as distinct in time; but the *direction* of the motion of a light under the same conditions can be perceived when the interval between the beginning and end of the motion is only .014 second. But one can reduce the distance so that the beginning and end of the motion are no longer distinguishable. This is especially easy on the lateral portions of the retina, where one finds that two disks so near together as not to be seen as two, are none the less seen to move with the slightest motion, the lateral portions of the retina being very sensitive to motor sensations. If a row of dots be viewed away off in indirect vision, the number or distances of the dots will not be seen, yet the addition or removal of a dot will be noticed instantly.

This sensation of motion as distinguished from the inference of motion has a lower limit. Aubert found that a motion slower than 1°-2° per second is not felt, which amounts to about a distance of 6-7 cones on the retina. Like ordinary sensations, these motor impressions leave an after-image. If a disk upon which a spiral is drawn be rotated, one will get the impression of a point moving towards the centre; if the disk be suddenly stopped, one sees a motion in the opposite direction. So, too, if after viewing such a rotating disk one casts the eyes on any object, that object, or that portion of it falling on the portion of the retina formerly stimulated, will be distorted, showing that these effects are retinal and not motor. If one views a disk with marked sectors rotating slowly, through a rotating disk with sectors cut out, one can so regulate the speed of rotation that the black sectors will not seem to be moving at all, being thrown back by the after-image upon the interruption of the sensation in the other disk.

That the after-image is confined to the portion of the retina formerly stimulated is shown by Dvorak, who had the different portions of his spiral rotating in different directions, and obtained an after-image corresponding to this difference. So, too, Fleischl found that after obtaining an after-image of a point moving horizontally, the projection of it on a series of vertical lines extended only as far as the original motion.

What will be the result if the two eyes have different motor-sensations? If you regard a rotating disk directly with one eye, and through a reversion prism with the other, you see opposite directions of rotation with the two eyes, giving rise to an uneasy feeling and no distinct after-image. If you close one eye you get its appropriate after-image. But most curious of all, if you look at the disk with one eye until fatigued, then close and look at a white surface with the *other* eye, you will see an after-image of the disk rotating in an opposite direction.

This holds equally well for the third dimension. A wheel rotating in the median plane is seen in the third dimension, and when suddenly stopped, the after-image is also seen in perspective. But this is obtained by a combination of the different after-images of the two eyes. A true after-image in the third dimension is not obtained. In the after-image only that portion of the nervous system is involved that aids in the perception of the adjacency of space impressions. In riding in the rear car of a train and looking backwards we see objects hurrying away from us. If the train stops we seem to be approaching the objects. In the former case the retinal impressions gradually grew smaller; now they by the after effect grow larger, and thus lead to the inference of our approaching them.

Again, it is found that if there is no stationary object in the field of vision, the minimum perceptible rate of motion is much raised; the threshold for motion becomes 10 times as high. It makes a difference whether the object moves across the retina or the eye follows the object across the field of vision; in the latter case the motion seems only about half as rapid as in the other. We have a more accurate notion of the motion of images on the retina resulting from the viewing of a stationary object while the eyes move, than we have of the motion of the eye muscles. If there is no object in the field of vision recognized as stationary, the perception of motion becomes vague; so in a dark room the movement of a light could hardly be seen.

We distinguish then between a sensation of motion which is immediate and is probably a subcortical function, and the conscious perception of motion by inference from various sensations.

Experimentalle Untersuchungen zur Amblyopiefrage. Dr. F. C. Müller-Lyer. Arch. f. Anat. u. Phys. 1887, p. 400.

Setting out with the idea of studying the phenomena of sight disturbed by disease, by studying normal sight as disturbed by experimental conditions, the author of this lucid article investigated four points, namely, (1) discriminative sensibility, (2) sharpness of vision, (3) color-sense, and (4) extent of the field of vision toward the periphery, in the three following conditions: (*a*) Simple weakness, (*b*) state of stimulation, (*c*) state following stimulation. Simple weakness of the eye can be paralleled for experiment by weakening the stimulus. The study of the first point under this condition is simply a retesting of Weber's law. After a set of careful experiments, the author found, as others have done, that Weber's law is not strictly exact. The discriminative sensibility is not constant, but depends on the intensity of the stimulus. The nearest mathematical expression for it (and that only an approximation) is that the former varies as the cube root of the latter. The second point was tested with printed letters, with the result of finding that the sharpness of vision follows in its decline essentially the same curve as the discriminative sensibility, but declines a little more slowly at first and a little more rapidly at last. The experiments on the third and fourth points coincide with the usual results, namely, that the limits of vision for green and red are first contracted, then those for yellow and blue; and the colors are finally lost in the same order, while the field for white remains uncontracted even with very considerable darkening. When the eye is stimulated from a source of light between itself and the object upon which it is fixed (condition *b*), it is found that when the extra stimulation increases in intensity, the discriminative sensibility declines more rapidly than the sharpness of vision, and that the disturbance of vision increases as the illumination of the fixated object is reduced, showing the eye thus stimulated to be delicately hemeralopic. Under such extra stimulation the visual field for white is concentrically contracted, the contraction depending in its amount, while the extra stimulus is constant, on the illumination of the object. The colors have their fields contracted and disappear in the order of their brightness, though this is not that of the extent of their fields in normal vision; blue, which then has next to white the widest field of all, may, under the influence of the extra stimulation, disappear, while all the other colors, except violet, are still to be seen. The results for the eye after stimulation (condition *c*) agree with those just given for condition *b*, except that with daylight illumination of the object fixated, colored vision is introduced (especially red-seeing and green-seeing), which brightens one color to the disadvantage of its contrasting color. For the diseases of vision with which the above conditions are comparable, as for the details of the apparatus and methods used in the experiments, the article itself should be consulted.

Die Umkehrung des Sehens und des Gesehenen mit Beziehung auf die gleichzeitige Seh-Abprägung. Prof. HOPPE. Pflüger's Archiv, XLIII, 1888, p. 295.

The "conversion of relief" in plane drawings, as in that which appears to be a half-open book, now seen from behind and now from in front, or like the Schroeder stair figure, has generally been explained as due to a change of conception in the mind of the observer, or to that helped out by ocular motion. Prof. Hoppe finds an additional factor in differences of the impression (*Abprägung*) of the image on the *macula lutea*. He presses the nativistic argument so far as to suggest that the *macula lutea*, in a certain way and to a certain degree, *knows* its own images. For proofs of his position the article itself must be consulted.

On Wundt's Theory of Psychic Synthesis in Vision. J. H. HYSLOP, Ph. D. Mind, XIII, p. 499, Oct. 1888.

After a preliminary explanation of the apparent location of stereoscopic images, Dr. Hyslop quotes Wundt's theory of psychic synthesis with qualified approval, and gives several interesting experiments (for the most part given in his letters to *Science* in the early part of this year) that in a measure confirm that theory. He finds in it, however, a confusion of two conceptions of innervation; the first making innervation very closely associated with actual muscular contraction and the discharge of nervous energy, the second being little more than the volitional impulse. The author's chief criticism is directed against the first conception. He points out complications into which the theory is driven when it tries to show why innervation of the internal and external recti for different degrees of convergence should give the notion of differences of distance in the third dimension, while that of the other ocular muscles, or even of the recti themselves for parallel motion, gives nothing of the kind. The difficulty of accounting by this theory for the localization at the same time of a pair of homonymous and a pair of heteronymous images is also urged. Since their place depends on innervation, there would have to be innervation at the same time and of the same muscles for different distances. Moreover, it can be shown by experiment that localization may vary with attention, while the position of the eyes and, presumably, the innervation that controls them, remains the same. Against the other form of the theory is urged that it makes a useless distinction of central and peripheral sensations in distinguishing those of innervation from others when all are really central.

Geschmacksprüfungen. KARL RITTMEYER. pp. 28. Göttingen Diss. 1885.

After reviewing the various opinions regarding the portions of the mouth cavity capable of perceiving taste, Rittmeyer made an independent investigation, thoroughly cleansing the tongue after each test, and avoiding contact with the edges of the tongue. He experimented upon ten persons, and found in every case a sensibility to taste outside the tongue—properly and especially (if not exclusively) upon two regions, a portion of the soft palate and the arcus glossopalatinus. Denoting a very pronounced taste sensibility by 1, a minimum sensibility by 4, and with 2 and 3 intermediate, the results for the four cardinal tastes in the average of ten persons were as follows:

For *sweet*, in nine cases the root of the tongue was 1, the edge 2, the tip 3, the soft palate 3-4, the arcus glossopalatinus 4. In one case the tip was 1, the edge 2, and the root 3.

For *salt*, in five cases the result was precisely the same as for the nine cases with sweet; of the remaining five cases, two differed merely in marking the soft palate 4 instead of 3-4, two differed by conforming to the exceptional instance with sweet, and one differed by marking the tip 4 and the soft palate 3.

For *sour*, the root of the tongue is marked 1 twice, 2 three times, 3 four times, and 4 once. The edge is marked 1 seven times and 2 three times. The tip is marked 1 once, 2 four times, 3 four times, and 4 once. The soft palate, 3 twice, 3-4 once, 4 seven times. The arcus glossopalatinus, 3-4 once, 4 nine times.

For *bitter*, the root is marked 1 nine times and 2 once. The edge, 2 nine times and 3 once. The tip 1 once and 4 nine times; the soft palate, 2-3 ten times. The arcus glossopalatinus, the same. This would make the root of the tongue best for sweet and bitter, the edge best for sour. Besides minor variations, one of the ten individuals shows a decidedly different distribution of sensibility from the other nine.

Next, the connection of the organs of taste with the nerves is discussed and illustrated by pathological instances. All agree in making the glossopharyngeal the taste-nerve for the root of the tongue, but the opinions vary regarding the connections of the anterior two-thirds of the tongue.

Next, the effect of various drugs upon the taste organs was tried, with the result of showing that *alumen crudum* and *zincum sulphuricum* in solutions of 1.25–1.5 per cent had the most decided effect. Testing the same subjects as before, and denoting a slight weakening of the sensory effect by A, a stronger one by B, a very strong one by C, and a total absence of taste by O, we have the following result:

For *sweet*, the root is marked A twice, B eight times. The edge is marked B once, C three times, and O six times. The tip, C once, and O nine times. The soft palate and arcus glossopalatinus each, B once, C once, and O eight times.

For *salt*, the root is marked A three times, C seven times. The edge, A five times, B four times, and C once. The tip, C ten times. The soft palate, B five times and C five times. The arcus glossopalatinus, C ten times.

For *sour*, the root was marked A once, B once, and C eight times. The edge, A five times, B twice, and C three times. The tip, A once, B seven times, and C twice. The soft palate and arcus glossopalatinus each, B seven times, and C three times.

For *bitter*, the root was marked A ten times. The edge, A twice, B seven times, and C once. The tip, O ten times. The soft palate, A once, B twice, and C seven times. The arcus glossopalatinus, A twice, B eight times. In other words, the root of the tongue loses its perception of taste least under the action of drugs, the edges next, then the soft palate and arcus glossopalatinus, and most readily the tip. Again, the root retains best its taste for bitter, next for sweet, third for salt, and last for sour. The edge retains the other three tastes about equally well, but is most liable to lose all taste for sweet. The tip retains best what it tastes best, the sour, but loses absolutely what little taste for sweet and bitter it normally has, retaining a slight taste for salt.

Finally, the application of a 2 per cent solution of cocain, besides inducing anæsthesia, does away with all taste for as much as half an hour, and longer if the application is allowed to remain. The taste for salt and bitter seems to come back first, that for sweet last.

Action des acides sur le goût. JOSEPH CORIN. Archives de Biologie, VIII, fasc. 1.

The relation between chemical action and sensation of taste is at the simplest with acids. Substances that taste acid are chemically acid. The investigation of this relation is the object of this very commendable research. When the sense of smell is excluded by holding the nose or by using very weak solutions of acid, the following have almost exactly the same quality, though different intensities of taste, and were used for experiment, namely, chlorhydric, phosphoric, oxalic, formic, sulphuric, acetic, nitric, tartaric, citric, hypophosphorous, malic, and lactic. Precautions were observed in making comparisons, to operate always on the same part of the tongue (the tip), to use the same quantity of acid, and to allow the same time for judgment. The tongue was used only when free of the effects of eating, drinking, or smoking, and was carefully rinsed. It was found best to test but a few substances per day, and these were of course varied in order, and arranged to avoid prejudgment. The number of taste organs stimulated, which Camerer found to affect the number of right guesses, does not seem to have been fixed beyond the using each time of the same quantity of the solution. In trying to fix the weakest solution of acids that could be distinguished from pure water, the experimenter found that with the same acid it varied at different times, probably with his own condition, from 3 to 35 parts in 10,000. It is possible, however, to compare weak solutions of the same or of different acids with a good deal of exactness if the experiments are made as nearly as may be at the same time. It was found that for solutions of chlorhydric acid ranging in strength from 15 to 25 parts in 10,000, a difference of 6 parts was distinctly perceptible; under exceptional circumstances, for solutions containing between 3 and 15 parts of acid in 10,000, a difference of 3 parts could be recognized. The portion of the research that bears directly on the connection of tastes with the chemical character of the acids, consisted in arranging variously proportioned standard solutions of the different acids in the order of their sourness. The results were consistent for both mono and polybasic acids, and are as follows: (1) the intensity of the acid taste is not the same for all the acids at the same degree of dilution, *i. e.* the same weight of acid diluted with the same weight of water; (2) the intensity of the taste is not proportional to the amount of replaceable hydrogen in the solution; (3) the taste of solutions containing each the same number of molecules of acid is stronger as the weight of the molecule is less. Whence it is concluded, (4) that "the intensity of the acid taste of a molecule of any acid depends on the relation of the weight of acid hydrogen contained in the molecule to the weight of the molecule." The order of the acids thus arranged is that given above. The experiments were all made by the author upon himself, and he recognized an educative process from the experiments in his power of discrimination. E. C. S.

Beobachtungen über die Geschmacksempfindungen nach der Zungenexstirpation. N. Cybulski and A. Beck. Transactions of the Academy of Sciences of Cracow, 1888; noted in Centralbl. f. Physiol. No. 12, Sept. 15, 1888.

These experimenters found in a patient whose whole tongue, including the basal taste papillae, had been removed, that there yet remained some ability to taste. The sensations of sweet, bitter and sour could be caused by touching the back of the throat or the mucous surface of the stump of the tongue with appropriate substances, though in the latter case they were only perceived when movements of swallowing were made. The taste of salt could not be excited.

Die Einwirkung der Kohlensäure auf die sensiblen Nerven der Haut. Goldscheider. Verhandl. der Physiol. Gesells. zu Berlin, Nov. 25, 1887.

When the hand is plunged into a vessel of carbonic acid, a sensation of warmth is felt. This increases for a time and then declines.

At the suggestion of du Bois-Reymond the author undertook an investigation of the phenomenon. After excluding more or less completely by experiment or well known physical principles the possibility of the sensation being due to the dampness of the gas, its conductivity, its heat capacity, its absorbent power, its setting free heat in its absorption in the moisture of the tissues of the skin and its causing an elevation of the skin temperature by dilation of the small blood-vessels, he concludes that it is really due to an actual chemical stimulation by the gas of the nerves of warm sensation.

Thermische Experimente an der Küchenschabe (Periplaneta orientalis). V. GRABER. Arch. f. d. ges. Physiol. XLI, abstract by Hermann in Jahresb. Anat. u. Physiol. Bd. XVI, Abth. 2, 1888.

The limits of temperature fatal to these roaches are —6° C. and 41°. With decreasing temperature, at about 5°, they lose locomotion and, if they remain at that temperature, other power of motion also. They will still respond, however, to strong stimulation. Below 0° they soon become paralysed, but recover more or less perfectly when warmed again. At —5° or —6° they die in from 10 to 20 minutes. Increasing temperature makes them more lively; above 37° they go into convulsions, and die slowly at 41°, though for five minutes or less they can bear 60°. Graber tested the temperature preferences of these animals by an apparatus of three connecting chambers, the two outer ones of which were of variable temperature. If the side chambers were both high, say 38°, the insects all stayed in the middle one. If they differed by about 2° and were still high, most of the animals chose the cooler. If the side chambers were both cold, they picked the warmer. The roughness and conductivity of the floor were of great influence. The "optimum" or temperature of greatest preference was about 26° or 28°, but at this very point the animals were frequently uninfluenced in their choice by wide differences of temperature. When offered a very hot chamber and a very cold one, they preferred the hot one up to about 39°, or only went into the other for a little while to cool off. When the hot chamber was yet hotter, they preferred the cold, even if below zero. Strange to say, they did not in these experiments remain in the middle chamber.

Die räumliche und zeitliche Aufeinanderfolge reflectorisch contrahirter Muskeln. Dr. WARREN P. LOMBARD. Separat-Abzug aus Archiv f. Anat. u. Phys. 1885.

To know a reflex act one must know the muscular contractions that enter into it and their order and extent in space and time. Such an analysis Dr. Lombard made for the reflex contraction of the muscles of a frog's leg. He found that the reflex called out by a continuous heat-stimulation was not a continuous contraction, but one broken by periods of rest; also that the order of contraction of the muscles in a series of reflexes was not constant; that, other things being equal, the number of muscles excited, and the length of time required for the stimulus to spread to all the motor roots, varied with the kind and intensity of the stimulus. From these he concludes that there must be somewhere in the central portion of the centripetal-centrifugal arc an apparatus that holds back the incoming excitation till it has reached a certain intensity and then transmits it to the motor roots; and that there is independent connection between the centripetal nerve and each motor root controlling muscles that enter the reflex. The difficulties involved in the older assumption that the order of contraction is fixed by the various rates of central conduction are avoided by supposing that the order depends on a difference of excitability of the structures connecting the sensory and motor roots. The grade of excitability would depend on chemical conditions, which can change quickly and in limited areas, and so produced the variable order of contraction found. The importance of chemical conditions is apparent in the strychnized frog where the differences of the periods of delay for the different muscles are abolished.

Relation de diverses experiences sur la transmission mentale, la lucidité, et autres phenomènes non explicables par les données scientifiques actuelles. CHARLES RICHET. Proceedings of the Society for Psychical Research, Part XII.

This lengthy article of 150 pages with so startling a title, coming from so prominent a scientist, is sure to attract one's attention. In a topic where so much bad method has prevailed, one expects much from a trained scientific thinker. Unfortunately this expectation is doomed to disappointment. M. Richet's application of the theory of probabilities to his results is very shallow, and the nature of his evidence often entirely too subjective. To begin with, his subjects are four hysterical women, for whose honesty we must be satisfied with Prof. Richet's declaration in their behalf. The first test consists in his willing one of his patients to go to sleep when the latter is at a house several hundred yards distant. Upon going to the house he hypnotizes the subject, who then informs him of the time during which he attempted to will her to sleep. The experiment is varied, but the time given by the subject is in Prof. Richet's opinion so often near the truth that chance fails to account for the successes. Again, hundreds of trials are made to transfer a simple drawing from Prof. Richet's mind to that of the subject. A large number of illustrations record the more successful cases, but the new fact that is emphasized is the discovery that the reproduction was almost equally successful when M. Richet himself was unaware of the character of the drawing to be transferred. This leads him to postulate a state of "lucidity" in which mental impressions are possible without the ordinary aid of the senses. Again, he experimented with a group of sixty drawings with normal subjects, and found on the average seven successful "transfers" in two hundred trials, while with his selected subjects he obtained twenty successes in the same number of trials. The subject while in the hypnotic state attempts to describe the disease of a patient, a lock of whose hair she sees; the descriptions are vague and do not impress the unprejudiced reader as at all noteworthy. Experiments in guessing cards were tried, but the number of successes was not above what chance would account for. This only sketches a small portion of this comprehensive study, which must be read in the original with account of precautions and the illustrations of results.

M. Richet enters upon his research with what appears, in the light of a sound logic, an utterly false notion, namely, that chance or a new force is the only explanation; entirely neglecting the great probability of our having overlooked a natural mode of explanation, such as the effect of unconscious suggestion. Again, he values the mere accumulation of evidence, as opposed to the stringency of the evidence, far too highly; and more important than all, when he comes to rule out the element of chance successes he fails of his purpose entirely. To begin with, the only type of experiment in which the success attributable to chance is exactly assignable is that with the cards, which proves entirely negative. In all other cases the action of chance is only roughly estimated, with a large element of subjectivity; and to judge from this article, M. Richet seems very readily disposed to see a marvel in every unusual event. In that portion of the article dealing with coincidences, the frequent though not the less unpardonable mistake is committed of confusing the chances of an event happening at a time determined upon *beforehand* by a third party, and the calculation of the chances *after* the event, without taking into account the prediction of the occurrence. Finally, the fact that success was obtained when the agent did not know the nature of the drawings is not an argument for "lucidity," but an argument against telepathy, and suggests that the subject succeeded in getting a sufficient idea of the nature of the drawing to obtain three times the normal number of successes.

Hat das magnetische Feld directe physiologische Wirkungen? L. HERMANN. Pflüger's Arch. XLIII, 5 and 6, April 24, 1888, pp. 217-235.

The psychologic interest in this paper centres about the alleged powers of the magnet in hypnotic phenonema. Prof. Hermann attacks the problem from a purely physiological side, aiming to discover whether the presence of a strong magnetic field in any ways influences the behavior of sensitive tissue under ordinary stimuli. After calling attention to the fact that in the literature of the subject one finds only negative results, when the results are trustworthy, he recounts his own experiments, which were directed mainly to four points. (1) Is there any difference in the minimal intensity of an induction shock that will cause the contraction of a nerve-muscle preparation, when that preparation is in a magnetic field and when it is not? (2) Is there any difference in the curve of contraction of such a preparation when placed in a magnetic field and when not? (3) Is there any difference in the minimal rate of stimuli that will produce tetanus under the two conditions? (4) Will the curve of tetanic contraction differ in the two cases? To all these questions, the answer obtained from numerous experiments, made with great precaution, is entirely negative. The magnetic field has absolutely no physiological effect whatever. Basing his position on these and similar results (for animals behave perfectly normally in a magnetic field; microscopic functions continue as usual; placing one's head between the poles of a magnet results in no sensation), he launches a severe criticism against the unscientific proceedings of the "hypnotists" who attribute a marvellous influence to the magnet, under conditions anything but conclusive. He emphasizes the extreme improbability of any such result, and regards all such anti-physiological announcements as utterly untrustworthy and an evidence of nothing but the careless observation of the reporter.

Psychometrische Untersuchungen. JAMES MCKEEN CATTELL. Inaug. Dis. Wundt's Philos. Studien, III (1866), pp. 452-492; Mind, XI (1886), pp. 220-242, 377-392, and 524-538.

(2.) *Wundt's Philos. Studien,* IV (1887), pp. 241-250. *Mind,* XII (1887), pp. 68-74.

In his discussion of the factors of reaction times, Dr. Cattell more than foreshadows the distinction of sensory and motor reactions, which Wundt has made so important in the last edition of his Psychology. He conceives that in the reactions of practiced subjects the brain processes are not chiefly those that attend perceiving and willing, but rather a kind of voluntarily prepared reflex. "That is," he says, in speaking of light reactions, "the subject by a voluntary effort . . . puts the lines of communication between the centre for simple light sensations (in the optic thalami, probably), and the centre for the co-ordination of motions (in the corpora striata, perhaps connected with the cerebellum), as well as the latter centre, in a state of unstable equilibrium." In case, then, of an incoming nervous excitation, a part goes on to the cortex and arouses consciousness, but a part also shoots off on the prepared lines and causes the immediate execution of the motion of reaction. Dr. Cattell generally used his gravity chronometer, (*vide* §2, p. 709, Vol. I of this JOURNAL), to control the giving of the stimulus. For the signaling of the reaction he used a telegraph key, a lip key, and a sound key. The signal was made with the first by raising the finger, and in the others by calling out. The time was measured by a Hipp chronoscope. The author and Dr. G. O. Berger, both somewhat experienced in psychological experiment, acted as subjects, and care was taken not to introduce irregularity by fatigue, etc. The simple reaction time for daylight reflected from white paper was found to be for B. 0.151 s., for C. 0.147 s., reactions with either hand being about equally quick, with the vocal organs about 0.030 s. slower.

The central stages of reaction time, *i. e.* perception time (*Unterscheidungszeit*) and will time (*Wahlzeit*), cannot be measured directly; the only safe way to study them is in their variations. If the manner of reacting remains the same, the will time should be nearly constant, and the independent variations of perception time open to study. In the first set of experiments on perception time, the subject was shown two cards, one black and the other white on a black background, and was required to react with the hand to the white alone. This gave. B. 0.207 s., C. 0.242 s. Subtracting from these the simple reaction time, on the assumption (which, however, the author makes with some hesitation) that all the processes of conduction, etc., are the same, there is left for the stages of perception and will, for B. 0.061 s., for C. 0.095 s., and dividing these equally between them (which cannot lead to gross error with such small numbers), gives for the simple perception time alone B. 0.030 s., C. 0.050 s. Calculation from the vocal reactions gives about the same results. Variations of the experiments by the substitution of colors or letters or words as stimuli, and by changes in the discriminations to be made, increased the perception time by different amounts. The perception time for pictures about one cm. square and, as the author conjectures, for the objects to which they correspond, was for B. 0.092 s., for C. 0.117 s.

The will time is studied by changing the manner of reacting. Instead of reacting to a designated stimulus in a single fixed way, stimuli of several kinds are used and a different reaction set apart for each. If two stimuli are used, reaction may be made to one with the right hand, to the other with the left. Using red and blue and yellow and green as such pairs of stimuli, B. took 0.018 and C. 0.034 longer than when a single previously determined motion sufficed. Such experiments were also made on letters with similar results, but the most numerous were made with vocal reactions—the color was named, the word called out, etc. In this case the variations in the manner of reacting are more numerous and the association of stimulus and reaction closer. Letters, figures, colors, words, and pictures were the stimuli, and interesting variations in their times were discovered.

Dr. Cattell sums up his measurements in round numbers, in thousandths of a second, as follows: Simple reaction time for light, B. 150, C. 150; recognition time for light, B. 30, C. 50; for a color, B. 90, C. 100; for a picture, B. 100, C. 110; for a letter, B. 120, C. 120; for a short word, B. 120, C. 130; naming time for colors, B. 280; C. 400; for pictures, B. 250, C. 280; for letters, B. 140, C. 170; for words, B. 100, C. 110. The author investigated with great care, as well, the effect of attention (concentrated, normal, and distracted), fatigue (making a very long special test), and practice, concluding that the first two are of less influence, at least with practiced observers, than has commonly been supposed.

(2) The last division of this research carries still further the application of time measurements to mental action. Four of the five groups of measurements deal with association or recollection, the fifth with the time of acts involving a judgment. The first group gave the time for naming pictured objects in a foreign language (for B. 0.172 s., for C. 0.149 s. longer than to do the same in the mother tongue), and for translating German words into English and *vice versa* (from less than one-fifth to nearly three-fifths of a second longer than for simply seeing and naming a word). For the second group the subject was required to give the country when a well-known city was given, the sum or product of given numbers, the language when an author was given, etc. These associations required from two-fifths to four-fifths of a second. The third group allowed more liberty of answer. When a country was given, the subject had to reply by a city in it, or when an author was given, by one of his works, etc. The times ranged from about two-fifths to one and one-tenth seconds. The associations of the fourth group were such as a thing with its parts or its uses, class name with examples of the class, a verb with subject or object, etc. The time required was from a little under three-tenths to a little over four-fifths of a second. The element of judgment was introduced in the fifth class by requiring an estimate of the length of single lines, or of the number in groups of them, or the relative greatness of great men. The times were from about one-sixth to about one and one-eighth seconds. For the exact figures, as for interesting peculiarities of association suggested by them, the article itself must be consulted. The mental processes measured, however, are not the same even in groups of the same general form; the average variations of the times found are very large, amounting in the corrected averages not infrequently to more than one-fifth of the whole; and the number of experiments is very small, often not more than twenty-six of a kind. These points, which the author indeed recognizes, leave this part of his research with hardly any

value more weighty than suggestiveness. It may very well be questioned whether the measurements have not been pushed to more complicated processes than can yet be approached with advantage.

E. C. S.

Ueber den Einfluss der Uebung auf geistige Vorgänge. Dr. G. O. BERGER. Wundt's Philos. Stud. V. 1. 1888.

The influence of practice was measured by its effect on the rapidity with which gymnasium pupils of different classes, and those of the highest class of a preparatory school, could pronounce Latin and German. The best five and the worst five in each of the classes were taken for the trial; the average age in the class from the preparatory school was 9; in the highest gymnasium class, 21.6. The test consisted in reading with the greatest rapidity first 100, then 500 words, and third, the first 100 words again at the normal rate. The Latin read was from Tacitus's Agricola; the German, from Goethe's Egmont. The improvement in the rate through the ten classes follows what may be assumed as the general law of the effect of practice, namely, a rather rapid quickening at first, followed by less and less gain as practice continues. The time for 100 words in the preparatory class, which had not as yet studied Latin, was 262 seconds; for the gymnasium classes respectively, 135, 100, 84, 79, 57, 54, 49, 48, 43. For German the times were 72, 55, 43, 37, 39, 28, 27, 26, 25, 23. The 100-word rate in Latin is 7 per cent shorter than that which can be kept up for 500 words; in German, but 3 per cent. The "normal reading" in the lower classes was a little quicker than the first reading because the words were a little familiar. The higher classes took longer for the second reading than for the first because they read for the sense. To set aside the possible objection that the increased speed was an evidence of increased mental quickness, and not the result of familiarity with the language, the gymnasium pupils were shown sets of five and of ten colors, and the time required to recognize and name them measured. The rates do not increase regularly with the increase in age, as they should do if the objection were valid. Granting the increased rapidity by practice, the question follows as to how practice has made the change. The gain appears to be chiefly in the overlapping of processes, as in Cattell's experiments (noted in the JOURNAL, I, p. 709), and in the size of the groups of words grasped at a time. The children in the preparatory school, for example, read Latin by syllables; those a little more advanced, by words; the highest, by phrases, as is testified by the kinds of errors made in reading at full speed, and by the less proportionate advantage shown by the boys of the higher classes in reading disconnected words.

Ueber die Reactionszeit für Erregung und für Hemmung. GAD, nach Versuchen des Herrn Dr. Orschansky. Verhandlungen der Physiol. Gesells. zu Berlin, No. 13-14, June 4, 1887.

The muscle selected for these experiments was the masseter, because its relaxation is not attended by the contraction of an antagonist. Its contractions and relaxations, by means of which the reaction times for excitation and inhibition were measured, were recorded by a double-branched apparatus, one branch of which entered the mouth on each side and pressed against the muscle, while a forward pair were adapted to recording with a Marey's drum. For comparison, the motions of the lower jaw under the action of antagonistic muscles were also experimented upon. The rhythm of most rapid contraction and relaxation proved at first slower in the masseter than in the others, but practice equalized the rate, though it seems to be effective only in shortening the stage of contraction. Such experiments, however, are not suited to determine the real reaction time for inhibition. When that was measured directly by reacting with relaxation to an electric shock, the inhibition time was found to be the same as that for excitation, and the equality continued through variations of the intensity of the shock, fatigue, alcohol dosing, etc. The simple reaction time for the jaw motions was, closing 0.15 s., opening 0.17 s.; for the masseter, contraction, before practice 0.25 s., after practice 0.15 s.; relaxation, before practice 0.30 s., after practice 0.14 s. Interesting experiments were also made on the variations introduced by the strength of the spring that pressed the arms of the apparatus against the muscle. If the subject intended to cause a slight motion and the spring was stiff, the reaction time was decreased; if under the same circumstances he intended to make a considerable motion, the time was lengthened.

Ueber Wiedererkennen. Versuch einer experimentellen Bestätigung der Theorie der Vorstellungsassociationen. ALFRED LEHMANN, Ph. D. Philos. Studien, Bd. V (1888), H. I, pp. 96–156.

Can all the phenomena of association be explained by the law of *contiguity?* This is the problem that Dr. Lehmann attempts to solve. From the standpoint of association by contiguity, recognition of simple impressions is possible, in the author's opinion, only under two conditions: first, that the memory-picture of a former sensation still exists, with which the later sensation may be compared; or, second, that a name or the like has been associated with the sense impression. The latter is not strictly recognition, but is so called. Dr. Lehmann's experiments, performed with sensations of light, cover both cases.

In the investigation of the first case, the different shades of gray, produced by means of rotating disks partly black and partly white, were employed. The disks were shown by means of a carefully prepared apparatus, in the following manner. First, a disk of normal shade was shown. After the lapse of an interval the normal disk was again shown; or a disk of lighter shade, or one of darker shade appeared. The observer judged whether the disk last shown was like or unlike the former one. In the first set of experiments only two disks were used. The interval was 30 sec. The normal disk was half black and half white, *i. e.* it = 180° black + 180° white. The other disk varied between 240° white + 120° black and 188° white + 172° black. Under these conditions, as the amount of white in the disks decreased, the average number of correct answers in each series of 30 experiments fell from 29 to 18 with one observer, from 27 to 17 with the other; the number of correct answers likely to occur by chance being of course 15. Thus as the difference between the normal disk and the light disk decreases, the number of correct answers diminishes. In another set of similar experiments three disks were used; and the light disk was always as many degrees lighter than the normal disk as the dark one was darker. Six series of 30 experiments each showed a decrease in the number of correct answers, especially as the disks approached one another in shade. Thus an increased number of possible impressions decreases the number of correct answers. Varying the time-interval, five series of 30 experiments each showed that, as the time increased from 5 sec. to 120 sec., the number of correct judgments decreased with one observer from 30 to 17, with the other from 21 to 17. Individual differences due to inclination, talent, and other personal conditions are here apparent. The effect of practice was also noticeable, the second half of a series generally showing more correct answers than the first half. Thus far, in the author's opinion, the theory of association by contiguity satisfies all demands as an explanation of the recognition of simple impressions. Dr. Lehmann further argues that on the hypothesis of association by similarity, the observer would more frequently recognize the normal disk (when three are used) than either of the others, because it is shown more often. But on the supposition of recognition by contiguity, provided the memory picture of the normal disk be distinct, no such difference would appear, but the probability of error would be just as great with one disk as with another; while if the memory picture of the normal disk be indistinct, it would be recognized less frequently than either of the others. The results of the experiments favor this view. B, with a clear memory, making only 107 errors, misjudged the normal disk 55 times, the other disks 52 times. L, with a vague memory, making 165 errors, misjudged the normal disk 109 times, the other disks only 56 times.

In the investigation of the second case—that of recognition by means of a name or designation—Dr. Lehmann used scales with black and white as the outer parts and a varying number of intervening shades of gray. First, one of these scales was shown; then, after an interval, the different shades were shown separately, and the observer judged of their place in the scale. Here it would be expected, if recognition occurs through a name, that, when the number of shades is not greater than the names of gray in daily use, nearly all the judgments would be correct, and that when more shades are employed, many errors would occur. Experiment confirmed this supposition. With a five-part scale—black, dark gray, medium gray, light gray, white, the number of shades of gray here corresponding to the names in common use—96.7 per cent of the judgments were correct; with a six-part scale only 70.6 per cent were correct; with a nine-part scale, only 46 per cent (the minimum number of correct answers to be expected by chance would be 37 per cent with a nine-part scale). The hypothesis that recognition here occurred by means of association with a name was further corroborated by experiments with a nine-part scale, where by simply associating a number with each shade, 75 per cent of correct answers were obtained. To obtain still further proof of recognition through a name, another set of experiments were performed similar to those first described, except that before each experiment the two disks used were simultaneously shown, thus enabling the observer to note the difference between them and to give them designations. This increased the number of correct answers. In these cases, if the name is the means of recognition, then the amount of difference between the disks, provided it be sufficient to be perceived, ought to have no influence upon the certainty of recognition. Nor should the time elapsing, nor personal conditions, nor practice have any considerable influence. The results of Dr. Lehmann's experiments gave support to all these inferences. Hence he concludes that the theory of association by contiguity explains all the phenomena of recognition, and that the theory of association by similarity, which cannot explain them all and sometimes is in conflict with experience, is superfluous. W. H. B.

The Senses of Animals. Sir John Lubbock. pp. xxix, 292. International Scientific Series. D. Appleton & Co., N. Y., 1888.

Having been obliged to look up a great deal of literature on the subject of the senses of animals, Lubbock has put together into a book the information laboriously arrived at, for the sake of making the path of the next explorer easier than his own has been; and he has thrown in some observations of his own, additional to those heretofore published, besides some acute criticisms of the reasoning of other observers. The result is somewhat heterogeneous, but it is interesting all the same; it is not necessary that every book that is printed should be a harmonious whole. The list of books and papers consulted by him is very long, but Graber's latest work on the brightness-sense and the color-sense of animals seems not to have reached him at the time he wrote. This is strange, because it bears the date of 1884; and it is unfortunate, because it may be considered as the only absolutely thorough and scientific experimental investigation of those senses in animals that has yet been made. Graber determined the absorption spectrum of all his colored glasses and colored solutions, and the exact intensity of the light which they transmitted; he found out how strong the preference of each animal was for brightness or for darkness before testing its preference for colors; he offered his animals the choice between only two compartments at a time, rightly considering that to ask them to bear in mind their sensations long enough to choose between five or six was putting too great a strain upon their mental powers; he made with each pair of colors two sets of experiments, once with one color the brighter and once with the other color the brighter. None of all these precautions were taken by Lubbock. Graber worked under many disadvantages and with much lack of means for procuring desirable apparatus; he speaks with real grief of the fact that Lubbock, with rich laboratories at his command, did not proceed in a more systematic fashion. As regards results, concerning bees, for instance, they obtain for preference-coefficients compared with red, respectively,

	R	Y	G	B	W
Lubbock,	1	1.02	.96	1.5	1.19
Graber,	1	2	2	6	18

The only agreement is that, of the colors, blue is the favorite. Lubbock finds that white is only slightly preferred to red; Graber that it is visited eighteen times as frequently. Graber finds besides that both blue and white with ultra-violet are three times as agreeable to bees as without ultra-violet.

The book contains much that is interesting on the instincts of some animals, especially bees, and on the intelligence which they occasionally, though rarely, show when obstacles are put in the way of their carrying out their usual plan of action. Some wasps always put exactly five half-dead caterpillars in the cell of a male grub and ten in the cell of the female. Does this show that they can count? If not, it shows that they can do something else which answers the purpose just as well. Ants always recognize the members of their own community, though they may be 500,000 in number; this is not done by means of any sign or password, for it can still be done when the one recognized is senseless from intoxication. Experiments which were taken as showing that bees have a special sense of direction are proved to be quite inconclusive; the returning to the hive must have been done by mere recognition of known objects. There is an admirable résumé of the discussion as to whether the eyelets of compound eyes give each the whole image or only a small part of it; the latter opinion is plainly made out to be the better one. Lubbock regards it as mysterious that the rods and cones of the vertebrate eye should point outwards instead of inwards, though he says that it has some connection with development. It is difficult to see how there could have been any other arrangement, when it is remembered that the vertebrate eye is first a bladder and then a double cup pushed forward from the brain, instead of being a depression in the outer integument. The reason for this development, according to Balfour and Carrière, is simply that the portion of the ectoderm which was destined to give rise to the eyes has, in vertebrates, already been drawn in to form the brain. Neither is it mysterious that animals should see ultra-violet rays of light which to us are indistinguishable from blackness. There is good reason to believe that the reason we do not see ultra-violet is because the ultra-violet rays are strongly absorbed by the refracting media of the eye; an animal with a smaller eye would naturally not suffer so much from this inconvenience. C. L. F.

Zum Mass der Schallstärke. Dr. PAUL STARKE. Wundt's Philos. Stud. V. 1. 1888.

In completion of a former study on the question of the relation of the height of fall and of the weight of a falling body to the intensity of the sound that it produces, the author reports the verification of his former results. He finds that within the limits of error the sound is directly proportional to the height, with a constant weight, and to the weight, with a constant height. The different results of former investigators arose from their neglect of the influence of the order in time of the standard sounds and those to be compared, and of the influence of Weber's law. The sounds were produced by the fall of ivory balls of 8.07 and 16.12 grams weight, on an ebony plate from heights of from 100 to 600 mm.

Tonstärkmessung. ERNST GRIMSEHL. Wiedemann's Annalen, No. 8b, 1888. Also *in extenso* in Programmabhandlung des Realgymnasiums zu Hamburg, 1888.

Starting from an observation of Lord Rayleigh's that thin plates in a resonant column of air tend to place themselves perpendicular to the axis of the column, the author has constructed a phonometer in which the degree of rotation of a thin disk of mica measures the intensity of the tone. The amount of rotation is measured by the deflection of a small mirror, as in a reflecting galvanometer. The tube in which the bit of mica hangs is closed at one end by a thin rubber diaphragm, and at the other by a piston, by means of which it can be adjusted to tones of different pitch. Cuts of the phonometer and curves representing the intensity of the tone of sounding pipes under different conditions are given, but the formulae for exactly connecting the amount of deflection with the intensity of the sound have not yet been reached.

Esperienze sopra i corpuscoli Vater-Pacini del mesenterio di gatto. FUBINI. Annali universali di Medic. e Chirurgia, Nov. 1887, noted in La Psichiatria, An. V, fasc. 4.

The experimenter spread the intestines and mesentery of a chloroformed cat upon a warmed glass plate, and after the animal had regained consciousness, stimulated the nerves of the Pacinian bodies. He took the dilation of the pupils as an index of painful sensation, and used for comparison those produced by the stimulation of a nerve of general sensibility. After testing with electrical, mechanical, chemical and thermal stimuli, he concludes, from the similarity of the pupil reactions in the two cases, that it is to the nerves of general sensibility that the Pacinian bodies belong. Such a relation has before been conjectured, but it cannot be held as yet demonstrated, if this experimenter has been fully reported. The responses of the pupils are too indirect and general an indication to establish the identity of the sensations in the two cases.

Influence dégénérative de l'alcool sur la descendance. A. MAIRET and COMBEMALE. Compt. Rend. CVI, p. 667, March 5, 1888.

These investigators, in prosecuting a research upon chronic intoxication in animals, have made a few very interesting preliminary experiments on the effect of alcohol on offspring. For the first experiment a vigorous and intelligent shepherd dog was given daily through a period of eight months, increasing doses of 72° absinth till he received 11 gr. per day per kilo of weight. This treatment produced hallucinations, illusions and dementia, with general paralytic troubles. When in this condition, but in a period when dosing was suspended, he was given access to a young, vigorous and intelligent female. She bore twelve pups; two were born dead, and none outlived 67 days. Three died from accident. The other seven suffered variously from epileptiform attacks, verminous enteritis, pulmonary and peritoneal tuberculosis, and besides, from lesions to be directly attributed to alcoholic degeneration—thickening of the skull, *sutures précoces*, adhesions of the dura mater to the skull, difference in weight of the two hemispheres, and fatty degeneration of the liver. The mother herself remained well. In the second experiment, a strong and intelligent spaniel bitch was given, during the last twenty-three days of gestation, from 2.75 to 5.75 grams of 72° absinth per kilo of weight. She first bore four pups, three alive and one dead, and, thirty-six hours later, two more dead. Of the three living ones, two were well formed but unintelligent; the third, a bitch, was less well developed, lazy, greedy, ungraceful in motion, short-winded, and too dull of smell to find her food in the dark. The offspring of this degenerate creature, though sired by a vigorous and intelligent dog, show the effect of the alcohol in the third generation. No absinth at all was given, but of the three pups that she bore, one died in a few hours, was club-footed, had several atrophied toes, deviation of the apex of the heart to the right, and other physical anomalies. Another died five days old, very thin and athrepsic, with the foramen of Botal still open. And the third at fifty days of age was reported intelligent, but is touched with *carreau* and has atrophy of the hind quarters. The degeneration is in this case, therefore, greater in the third than in the second generation. E. C. S.

III.—ABNORMAL.

Apraxia and Aphasia. Dr. M. ALLEN STARR. Medical Record, Oct. 27, 1888.

The possibility of successful surgical treatment of many brain troubles has given an immense significance to all mental symptoms that can point to the seat of the lesion. It is with the practical aim of stimulating the observation and recording of such symptoms that Dr. Starr makes his exposition of apraxia, aphasia, and related states. The term "apraxia" is relatively new in neurology, and is used to cover a class of mental disturbances of which "psychic blindness" and "psychic deafness" are the best known examples. The physical basis of the concept of any object is an associated group of the residua of the sense impressions of it, retained in the various sensory centres of the brain. As the result of localized brain disease, one or more of these centres may be destroyed, or suffer a more or less complete severance of its connections with the rest. If the disease affects the visual factor, the patient may be able to see an object before him, but only know by inference from its giving utterance to a human voice that it is a human being. Or if the disease affects the auditory factor, he may be able to hear and recognize music, but not to understand words said to him. Apraxia is, in general, the "inability to recognize the use or import of an object"; and there may be as many forms of it as there are senses. Like aphasia, it is caused, so far as known, only by disease on the left hemisphere in the right-handed. In every educated person there is beside this concept-group, a word-group associated with it and made up of the residua of sensations connected with the heard, spoken, seen, and written word. By disease of the elements of this group the various aphasias, word-deafness, word-blindness, agraphia, etc., are produced; by the severing of some of its connections, paraphasia. The author gives a brief account of these, with a schedule of the points to be examined in making a diagnosis of them; also two tables analysing 15 cases of apraxia, and four cases from his own observation of word-deafness, word-blindness, paraphasia, etc. The article gives in brief space much matter of interest to the psychologist.

Versuch einer Darstellung unserer heutigen Kenntnisse in der Lehre von der Aphasie. ERNST MALACHOWSKI. No. 324 in Volkmann's Sammlung klinischer Vorträge.

A great point in such a presentation is clearness, and in this the author succeeds admirably. With a frequent use of schematic diagrams he demonstrates the centres involved in speech and their connections, discusses the possible lesions of both and the resulting language symptoms, and finally makes such connections as are at present possible between the diagrams and actual brain structure. In his presentation he generally follows Wernicke.

Verhalten der musikalischen Ausdrucksbewegungen und des musikalischen Verständnisses bei Aphatischen. H. OPPENHEIM, in the Charité Annalen, XIII Jahrg. 1888; reviewed in Neurol. Centralbl. No. 18, Sept. 15, 1888.

In 16 cases of aphasia the author found 11 in which the ability to sing and to understand melodies remained, in spite of a more or less complete loss of active speech and, in most, of the understanding of spoken words. A careful analysis of the cases, however, revealed that almost every one retained the language of emotion, and to some extent mechanical automatic speech. By the presence of these the author explains the preservation of the musical capacity. The other 5 cases were not worse than some among the 11; they nevertheless showed loss of musical understanding, though two at least were known to have been able to sing. The difference of the groups leads the author to the conclusion that musical capacity may perhaps be located in a distinct area of the left hemisphere. In support, by analogy, he recalls a case observed by himself in which the memory images of numbers were destroyed by disease in the right hemisphere without disturbance of speech.

Ein Fall von Alexie mit rechtsseitiger homonymer Hemianopsie ("subcorticale Alexie." Wernicke). DRS. L. BRUNS and B. STÖLTING. Neurol. Centralbl. No. 17. 1888.

The patient, a man 51 years old, had an apoplectiform attack, with disturbance of vision and right paraesthesia, but without definite paralysis. He showed a little difficulty in naming objects, occasionally was unable to do so, was a little paraphasic, and for a few days somewhat disturbed mentally. About a month after the attack he was carefully examined by the writers. His vision was found right hemianopic; he was a little awkward in the finer movements of the fingers of his right hand. There were transient signs of psychic blindness, scarcely noticeable paraphasia, and possibly a slight weakening of mind. But he still had difficulty in naming objects. Occasionally he could recall their names after touching them, but sometimes had to resort to circumlocution. He could easily repeat the names when given them, or point to the object when he heard the name. He [illegible] read [illegible] letters [illegible] not [illegible] when told to do so. He could [illegible] script letters [illegible] tions, and somewhat [illegible] script. He also knew the Arabic figures, [illegible] the script letters he [illegible] a number of times [illegible] writing; when [illegible] else, he recognized the letters [illegible] when the forms of [illegible] He [illegible] from [illegible] read what he had written. [illegible] with almost [illegible] depending on the [illegible] writing.

After presenting their case, the authors discuss the kind of lesion that should correspond with such a set of symptoms. They assume for the discussion generally accepted tentative schemata, and, on the authority of Wilbrand and Wernicke, the principle that optical images, including those of words and letters, are preserved in duplicate in the right and left optical centres. It is interesting, without going further into the discussion, to remark that the preservation of the names of objects, while those of printed letters and words are lost, depends on the association of other sensations with that of sight in the case of the objects. The sight of the object calls up the associated sensations—for example, touch sensations among the the rest, or they are directly excited by handling the object. The connection between the touch centre and that of speech is uninjured, and makes possible the giving of the name. The image of the printed letter or word, on the contrary, has no other associations (or almost none), and so when the direct connection between the optical and speech centres is broken there is no byway by which the latter can be reached. Written letters and words have an advantage in associated motor sensations, and by means of them, as in the case of this patient, the spoken equivalent may be reached.

Acrophobia. Dr. ANDREA VERGA. Translated in Am. Jour. Insanity, October, 1888.

In this paper, read somewhat over a year ago at the congress of alienists at Pavia, the author makes confession of his own extreme dread of high places. Though fearless of the contagion of cholera, he has palpitations on mounting a step-ladder, finds it unpleasant to ride on the top of a coach or to look out of even a first-story window, and has never used an elevator. Merely thinking of those that have cast themselves from high places sets him tingling in the calves of his legs, his heels and the soles of his feet, or in his neck. He even experiences physical discomfort at the thought of the earth spinning through space and the imaginary possibility of the centrifugal overbalancing the centripetal force. He finds this fear growing upon him with years, as sight and hearing and the courage that they give begin to fail; even the small feats of walking in high places that were once possible to him he can no longer perform. The translator of the article also confesses the same fear. In his case the special dread that he feels on seeing a child near an open window has been given a peculiar force by the fatal fall from a window of the child of a friend. There are no doubt many other cases where the feeling has been caused or intensified by such shocking experiences.

A Rare Form of Mental Disease (*Grübelsucht*). CONOLLY NORMAN. Journal of Mental Science, October, 1888.

As the name of this disease signifies, the sufferer from it torments himself with endless questionings and needless investigations. The case here related was that of a married woman, thirty-two years old, who had been prepared for disease by excessive child-bearing and nursing in unfavorable circumstances. The trouble began in feelings of suffocation on waking, and the fear that if she did not rise at once the walls would fall in, she should go crazy, or something else dreadful would happen. After a time she began to feel compelled to examine any bit of straw or paper or glass that she saw. In the street she must find out what any scrap of written or printed paper was and to what it referred. Once having passed some such bits in the evening she was unable to sleep, and finally had to waken one of her sons and go and get the papers. In doing so she shut her eyes to avoid getting into the same trouble again. For the same reason she stayed as much as possible in a darkened room. These feelings materially interfered with her housework. Thyme in the soup led to such questionings as these: "I asked myself, is that a little bit of thyme? It might be something else. That other little bit—is it thyme? I shall never be sure that all these little pieces are thyme. Can there be anything else but thyme in it? What *is* thyme?" She had to read every word in the newspaper. She was oppressed by a sense of the unreality of things; was unable to act with decision. Yielding to the impulses brought a temporary sense of relief, but denial of them led to nervous attacks, which also followed slight shocks, the necessity for prompt action, or even came uncaused. These began with a fearful sense of something to happen, of something wrong, and of helplessness, and went on to confusion, pain in the vertex, buzzing in the ears, and finally trembling and an outburst of perspiration. She was painfully conscious of her trouble and feared insanity. With cessation of nursing, etc., nourishing food and tonics, and the encouragement that she would get well, she gradually improved, was able to get control of her impulses, and finally made a good recovery.

Ueber psychische Infection. ROBERT WOLLENBERG. Archiv f. Psychiatrie, Bd. XX, H. 1.

From the study of a large number of books and articles, the author gives a comprehensive statement of present information on the subject of what has been known in France as *folie communiquée*, *folie simultanée*, *folie similaire*, *folie à deux*, *à trois*, etc., and in Germany as *inducirtes Irresein*, *communicirter Wahnsinn*, *Simultanwahnsinn*, and *psychische Contagion*, *Ansteckung* or *Infection*. The conditions that favor transfer of the insane ideas, the kinds transferred, the prognosis and treatment, etc., are discussed, and illustrated by brief abstracts of cases, often by many. In conclusion the author shows at length, in an interesting case of his own, how delusions of persecution grew up in the minds of two sisters, and were by degrees accepted by their father. To the article is appended a bibliography of 103 titles, of which the first 43 relate to psychic epidemics, the remainder to sporadic cases affecting only a few individuals.

Ueber Intentionspsychosen, mit Nachtrag. LUDWIG MEYER. Archiv f. Psychiatrie, Bd. XX, H. 1.

Cases in which, as a result of psychic shock, associations of such a nature are formed that the most trivial objects or events call up vast psychic disturbances, are not very rare. For this general group of cases, Meyer proposes the term "Intentionspsychosen," because in them the most striking feature, both to the patient and the physician, is morbid attention (*intentio*) to some immediately present sensation, having in mind also certain analogies to "Intentionstremor," and the dependence of the latter on intended movements. Both sensory and motor cases are included, and of them a number of illustrative cases are given—of the first, a lawyer who found himself prevented from writing in the presence of others, by attacks of dizziness, with palpitations and trembling of the hand; three ministers who became dizzy, as at a great height, when they mounted their pulpits; two agoraphobiacs, whose special difficulty was with open spaces that were paved; two locomotive engineers who were unable, on account of somewhat similar attacks connected with their business, to do their work. On the motor side the disturbances appear as obscure compulsions or inhibitions, like those experienced in high places—a merchant sees the bread-knife and his child, or later, the child alone, and has an almost uncontrollable impulse to cut its throat; a more unfortunate peasant, under similar circumstances, actually murders his child; others are urged to acts of a perverted sexual nature.

Le pazzie transitorie. S. VENTURI. Napoli, 1888. pp. xii, 94. Abstract by Sommer in Neurol. Centralbl. No. 15, 1888.

In this short treatise on temporary insanity Prof. Venturi has collected 56 cases so diagnosed. After rejecting 24 of these that had shown earlier signs of psychical irregularity, he divides the remaining 32 into six groups as follows: 1. Passionate—one incompletely observed case, corresponding to pathological anger in that it followed an insult and was followed by deep sleep and amnesia. 2. Impulsive (6 cases)—a single senseless, generally violent deed, followed at once by a short period of delirium without further violent inclinations. 3. Hallucinated (2 cases)—confused delirium following sudden hallucinations of sight. 4. Somnambulic (4 cases). 5. Melancholic (2 cases). 6. Maniacal (17 cases). Direct predisposition was found in 7 of these 17, and indirect in 11; prodromic headache and oppression were present twice. The attack lasted 6 hours in four cases; 8, 10 and 12 hours in two cases each; 3, 4, 5, 13, 14, 15 and 24 hours in a single case each. There were attempts at murder four times, at suicide twice, violence to bystanders seven times, and mere destructiveness four times. In three cases certainly there was recurrence of the attack. Deep sleep and amnesia followed in these 17 cases, though in those of the other forms the sleep was wanting eight times and the amnesia three. Prof. Lombroso furnishes a commendatory introduction, though still holding to his opinion as to the epileptic origin of temporary insanity.

On the Pathology of Delusional Insanity (Monomania). JOSEPH WIGLESWORTH. Journal of Mental Science, October, 1888.

The pathological distinction that the author suggests between mania and monomania is incisively stated as follows: "Mania begins from the top, monomania from the bottom." In mania the regulative control of the highest centres is disturbed, and the lower centres are over-active in consequence; in monomania the trouble is in the lower centres (including under that term the cortical centres primarily concerned in perception, and those below them), or still nearer the periphery. The intellect is, at least at first, untouched, but is misled by the abnormal sensory or perceptive data furnished to it from below. The constant association of hallucinations with typical delusional insanity, and the frequency with which the delusions can be traced to them, make the presumption strong that the disease that causes the hallucinations is the tap-root of the insanity. That such is the case is further made probable by certain cases of locomotor ataxy, in which the development of delusions may be followed concurrently with the advance of the disease in the peripheral nerves.

Presidential Address delivered at the Annual Meeting of the Medico-Psychological Association, Edinburgh, August 6, 1888. T. S. CLOUSTON. Journal of Mental Science, October, 1888.

The major part of this address was devoted to Secondary Dementia. General mental death is so characteristically the "goal of all insanities," that "mental disease may be defined as 'a tendency to dementia'"; and so common is it, that two-thirds of the asylum population of Great Britain are more or less demented, and two-fifths of the new cases become so. Its most typical secondary form is that which follows the insanities of adolescence. As idiocy is a failure of the brain in its period of growth, so the dementia consequent on these insanities is a failure of the highest brain tissue at the last stage of its development. The disease is not to be explained by the degenerative action of previous acute mental disease, nor by circulatory changes. It is strictly the result of bad heredity; there is a "tendency to dementia from the beginning." To know what this goal of insanities really is, and to prevent their reaching it, is the problem of psychiatry.

The address was eminently successful in calling out discussion (*vide* the report of the proceedings in the same number of the *Journal of Mental Science*). It was discussed by Drs. Tuke, Savage, Wiglesworth, Ireland, and others, almost every speaker taking issue with Dr. Clouston on one point or more.

IV.—MISCELLANEOUS.

Memory and its Doctors. Dr. E. C. PICK. London, 1888.—*Memory. What it is and how to improve it.* DAVID KAY. London, 1888. "*Loisette*" *exposed, together with Loisette's Complete System of Physiological Memory.* G. S. FELLOWS. New York, 1888.—*Memory Systems, Old and New.* A. E. MIDDLETON. With a bibliography and other matter, by G. S. FELLOWS. New York, 1888.

The appearance of these volumes testifies more than to anything else to the great popular interest in psychological matters, especially when any practical advantages are to result; for the idea has not yet been entirely abandoned that some royal road to knowledge is still to be found, some mysterious method of which a favored mortal possesses the key still to be revealed. While psychology is supposed to hold some definite position regarding such themes, hardly any confidence is placed upon these opinions in a matter of practical application. Thus the professional memory-teacher gains success from a public that ought to know better.

Dr. Pick's little volume presents quite modest claims. He bases his system upon natural acquisition by real labor, not hampering the pupil by associations artificially imposed, but simply advocating the good effects of method and an attention to one's associations. Especially do the sound portions of his treatise become prominent when we contrast them with the shallow attempts of his predecessors, of whom he gives a concise and convenient account. There we read of associations of dates with the rooms of a house, with harsh sound combinations and the places in a magic square. Even a memory pill and a memory diet was advocated. Prominent examples of mnemonic feats are also entertainingly given. This primer can be recommended as a pleasant introduction to the topic, and is especially interesting by the success of Dr. Pick's teaching, and the further fact that it is from him that Loisette has borrowed so largely without acknowledgment.

Mr. Kay's more comprehensive work will also find a large public. He approaches the problem from a broad psychological point of view, with no haste to reach astounding practical results, and a sound interest in the educational value of psychological principles. He begins with the physiological concomitants of memory, devoting a chapter to the relation of body and mind, and others to the description of the senses and their functions, the nature of mental images, the rôle of the unconscious, and the like. The dependence of a sound memory upon close attention furthered by a living interest, upon active repetition, upon intimate association with centres of interest, are all well described, with a wealth of references to general psychological literature. It is a pity that his physiology is at times not strictly accurate, and still more so that he seems to be unacquainted with the recent German contributions to the topic.

The last two volumes are of greater popular than scientific interest. They are called out by the ridiculous pretensions and peculiar methods of "Loisette." The pledge to secrecy which he imposes is here disregarded and the entire lesson papers printed in full. This may have the good effect of showing the folly of trusting one's mental culture to the guidance of so artificial a system. The exposure led to the withdrawal of the book from the market by legal procedure, and to the publication of the last work on the list, in which the account of the proceedings is given. Mr. Middleton's account of memory systems is convenient but superficial. Great credit is due Mr. Fellows for his useful bibliography of the subject.

J. J.

Memory. Its Logical Relations and Cultivation. F. W. Eldridge-Green. London, 1888.

In opposition to most physiologists, the author endeavors to prove that memory is "a definite faculty," having "its seat in the basal ganglion of the brain." Rejecting phrenology, the author nevertheless comes dangerously near the position of the phrenologists, by dividing the mind into a great number of faculties. The "faculty" of memory he divides into "sensory memory" and "motor memory," and locates the former in the optic thalami, the latter in the corpora striata. Rules are given for the cultivation of both forms of memory. Those relating to the motor memory have to do with the learning of co-ordinated muscular movements.

Il fenomeno della ricordanza illusoria. Francesco Bonatelli. Rendiconti della Reale Accademia dei Lincei. Vol. IV, fasc. 4, February 19, 1888.

As an example of an illusion of memory, the author relates a dream which he seems to have had more than once. He dreamed of occupying a certain set of apartments, and each time remembered having lived in them years before; they were, however, on waking reflection, entirely different from any he had lived in. That this was not a case of recollection from dream to dream he believes, because with this exception his dreams have no similarity one with another, and because, in the waking state also, one is sometimes convinced that circumstances in which he has certainly never been before, are a repetition of others experienced in the past. In explanation of both phenomena he suggests that this conviction arises from an obscure emotional accompaniment of the perception. In peculiarly excitable states of the nervous system (as in vivid dreams, or when one is in strange places), parts of actual perceptions, as is normally the case, pass out of the focus of consciousness, and returning an instant later, meet changed conditions into which they do not fit, and therefore appear to be recollections. This rapid passing out of and into the focus of consciousness (or the physical concomitant of it) is not perceived, if we conceive the author rightly, but gives rise to the emotional accompaniment just mentioned.

Association by Contrast. M. PAULHAN. Revue Scientifique, Sept. 1.

The general law, applicable as well to higher states of consciousness as to sensations, is formulated, claiming that every psychic state tends to be accompanied (simultaneous contrast) or followed (successive contrast) by an opposite state. In sensation, the phenomena of complementary colors, of warmth following a sensation of cold, are typical. In motion, every contraction of a muscle involves the contraction of the antagonistic muscle. When moving and suddenly stopped, we seem to be going in the opposite direction. In the sphere of judgment, alternatives are ever present, an argument *pro* calls up another *con*. A vacillating temperament is characteristic of some types, while in the hypnotic subject it is strikingly absent. Morbid instances arise in which every idea realises its opposite, with alarming results. Again, depression follows joviality, and even the alleged phenomena of "psychic polarisation" would come under this law. Examples from all phases of psychic activity are brought together to show the wide bearings of the law of contrast. J. J.

The Geographical Distribution of British Intellect. Dr. A. CONAN DOYLE. Nineteenth Century, August, 1888.

Following the line of investigation inaugurated by Mr. Galton, Dr. Doyle examines the relative fertility of distinctive portions of the British Kingdom with reference to the production of celebrities. The degree of eminence recognized by Dr. Doyle is lower than that usually treated in such researches, and includes such as would deserve mention in a standard biographical dictionary like "*Men of the Time*" and yet rank higher than local celebrities. He selects about 1150 such men eminent in literature, art, music, medicine, sculpture, engineering, law, etc. These are found to contain 824 English born, 157 Scottish and 121 Irish, while 49 were born abroad. England would thus have one celebrity to 31,000 of population, Scotland one to 22,000, and Ireland one to 49,000. Wales, if counted separately, would have one to 58,000. London produces much more than its share of eminence, claiming 235 of the 824 Englishmen, or one to every 16,000 of the population. Dublin shows still better with 45 celebrities, one to 8500, and Edinburgh leads easily with 46, or one in 5500. While the chief cities are thus the intellectual centres, Dr. Doyle thinks the very greatest intellects come from the country. London is especially strong in artists and men of science. The standing of the various counties is detailed, making the eastern and southern counties superior to the northern and midland, "while that portion of Scotland which lies between the Forth and Clyde on the north, and the English Border, is in the proud position of having reared a larger number of famous men in the later Victorian era than any other stretch of country of equal size." Other conclusions are "that agricultural districts are usually richer in great men than manufacturing or mining parts." And that, "if a line be drawn through the centre of Lincolnshire, it will be found that the poetry of the nation is to the southern side of that division"; it being regarded that, with a few notable exceptions, music, poetry, and art reach their highest development in the south, while theology, science, and engineering predominate in higher latitudes. J. J.

Facts and Opinions relating to the Deaf, from America. ALEXANDER GRAHAM BELL. London. 8vo, pp. 196.

This report to the Royal Commission of the British Government to inquire into the condition of the deaf, is of great value to students of this interesting class of defectives. Five questions are treated, mainly by the statistical method and the collation of the opinions of experts. 1. Visible speech. The fact that of 31 institutions in which it has been introduced it has been continued in only 17, argues against its universal applicability. 2. The development of latent powers of hearing in the partially deaf is ably discussed, with the result that the future holds out bright prospects in this direction. 3. The most important topic is that of the heredity of the deaf-mute as a class. Here the experience of superintendents of asylums goes to reducing the evil effects of intermarriage, some holding that the additional happiness thus brought about is more than a compensation for the slightly increased chances of a deaf offspring; others holding that consanguinity is a more potent factor than deaf-mutism, while still others make a difference between the congenitally deaf and those who become so later in life. The scientists, on the other hand, are unanimous in their agreement with Prof. Bell's position that the marriage of the deaf-mute with the deaf-mute is an ever increasing factor in the production of deaf-mutism, and that, if continued, it must end in establishing a deaf-mute variety of the human species. 4 and 5. Under these heads various usages and modes of instruction of different schools are summarized. J. J.

A Method of Examining Children in Schools as to their Development and Brain Condition. FRANCIS WARNER. Brit. Med. Jour., Sept. 22, 1888.

In the rapid observation of children in these particulars, very much can be learned by attention to two classes of facts: "(*a*) the form, proportions, and texture of the visible parts of the body; and (*b*) the signs of action of the central nerve-system, as seen in the muscles producing movements or attitudes or balances of nerve-muscular action." The first shows the development and nutrition; and in the condition of the special features often lie indications of mental weakness. The second shows, in variations from the normal, nerve-muscle weakness, fatigue, and excitability. Besides such things as these and starvation, the doctor has found hare-lip, congenital cyanosis, rickets of the skull, brain disease with congenital syphilis, all grades of idiots, and, with the help of the teacher, *petit mal.* In the Day Industrial School, of Liverpool, 14 per cent of the

281 children showed defective development or nervous symptoms, but there was little exhaustion. Of the 106 children in one school for truants, 40 per cent were defective ; of the 47 in another school, 6 boys of eight pointed out as "specially bad or troublesome" seemed to have some physical basis for it, and ten "good, quiet and decent" boys showed signs of nerve weakness.

Eye-mindedness and Ear-mindedness. JOSEPH JASTROW, Ph. D. Pop. Science Monthly, Sept. 1888.

This paper is an interesting résumé of present information, increased from the author's own observation, on the effect on the mental complexion of a predominance of sight, hearing, or, more briefly, of touch. An eye-minded man learns easiest and does his best work with his eyes, an ear-minded man with his ears. The importance of regarding these differences in the conduct of mental life, either one's own or that of pupils, sets in a practical light the methods of determining the dominant sense, to which Prof. Jastrow devotes considerable space.

Genius and Precocity. JOSEPH JASTROW, Ph. D. Journal of Education (London), July 1, 1888.

This paper follows the lines of that of Sully (*Nineteenth Century*, June, 1886) ; but by stricter definitions of precocity and greatness, the author is able to go a step beyond the connection there demonstrated between precocity and eminence. He shows that among men of transcendent greatness (men of action and statesmen are excluded in both papers), the proportion of the precocious is nearly twice as great as among the merely eminent. An examination of the biographies of the specially precocious shows that while they produce work earlier than other great men (at about 15⅓ years on the average), they do not do great work earlier (29⅓ years), and do their greatest work, if anything, later (46⅓ years as against 44½), and that they do not die earlier. On the other hand, if the list is made still more exclusive, reduced to veritable *Wunderkinder* (the numerical basis being now, of course, small), the age of producing work of these degrees of excellence is decreased, for the great work and the greatest work, by about four years, and for the age at death by as many as six.

Insomnia and Other Disorders of Sleep. HENRY M. LYMAN. pp. 229. W. T. Keener, Chicago, 1885.

To the present generation, books on sleep and its disorders should be specially welcome. This one opens with a discussion of the nature of sleep, giving an elementary statement of the theories of Obersteiner and Pflüger, then passes to insomnia and its treatment, and adds something on dreams, somnambulism, and hypnotism. The discussion is not very thorough, the most useful portion being the first ninety pages. Beyond this, the anecdotal method of presenting the facts is adopted, and while this is usually entertaining, it is not always very valuable. Perhaps our information regarding sleep has not yet fairly passed from the descriptive phase with which the science of any subject begins, but there is certainly enough experimental work to be discussed to furnish the basis for a somewhat less poetical and more scientific book than the one in question.

Aerztliche Philosophie. Dr. G. E. RINDFLEISCH. Festrede zur Feier des dreihundert und sechsten Stiftungstages der Königl. Julius-Maximilians-Universität, gehalten am 2ten Januar, 1888. Würzburg: Commissions-Verlag von Georg Hertz. 1888. 20 pp. 4to.

This address by the Ordinary Professor of Pathological Anatomy, Pathology and History of Medicine at Würzburg is intended to express to young physicians opinions concerning the medico-philosophical questions of the present. The author declares his pity for those "who, with the necessary recognition of mechanism in all natural processes, have lost the spirit of a non-materialistic view of nature," and excludes himself from their number. Though we may measure the phenomena presented in time and space, still "on all that is there is the mark of infinity." And this "infinity" is not compassed by the circle of our measurements and determinations. Yet all straining after the unknowable on the part of the medical investigator is to be lamented. "Earnest, upright and conscious reserve toward the eternally undiscoverable, and indefatigable labor in the investigation and use of what is accessible to our understanding," according to the author, expresses the true scientific spirit. It is disregard of this spirit, he thinks, which has brought discredit on the history of medicine. A crass materialism and a superstitious spiritualism are both one-sided products of human weakness. "Neo-Vitalism" is selected as the theme for the positivistic text given above. The history of the Cell-Doctrine is briefly sketched and its present condition outlined. The conclusion is that no decisive philosophical datum has resulted from this line of investigation. Aside from its word of caution to students of biology, and its emphasis of reserve toward ultimate questions, and of loyalty to exact methods, the address has no special significance.

D. J. HILL.

Einleitung in die Psychologie nach kritischer Methode. PAUL NATORP. pp. 129. Freiburg, 1888, J. C. B. Mohr.

The professed object of this brochure is to state, not to solve, the problem of psychology. It is written from the purely introspective and speculative standpoint, with an entire unconsciousness of what has been done by Wundt and his school to bring the subject within the reach of experiment. There is a brief section discussing the question whether psychology can be made a natural science or not, but even in this there is no approach to a recognition of experiment. The work has no interest outside the technical investigation of transcendentalism.

NOTES.

Moderate illumination of the peripheral parts of the retina by light concentrated with a lens on the sclerotic increases the acuteness of vision in most normal eyes. Letters are made to appear blacker, and some not before legible become so. Only when the illumination is considerable does it interfere with vision. In a number of eye-diseases, however, the least side illumination produces disturbance at once.—(Schmidt-Rimpler, *Ber. d. XIX Vers. d. ophth. Gesellsch. zu Hamburg.*)

Charpentier gives, in the *C. r. Soc. de Biologie*, Mai 19, 1888, the results of experiments showing that the difference in brightness of two neighboring illuminated surfaces can best be seen with the central parts of the retina, except a less sensitive central spot. The discriminative sensibility falls off rapidly toward the periphery. It is better, however, on the outside than on the inside of the field of view, and above than below.

The same experimenter (*ibid.* Mai 16, 1888), taking neighboring surfaces, one illuminated continuously, the other instantaneously, adjusted the sources of light so that the surfaces appeared equally bright, and then weakened both illuminations equally. The continuously illuminated surface appeared the brightest, and indeed when the illumination was very much reduced was the only one seen. When both surfaces were illuminated for different lengths of time and yet made to appear equally bright, and the size of their images was then reduced by a concave lens or by withdrawing from them, the one most briefly illuminated appeared the clearer and brighter.

The number of the visual units in parts of the retina has been redetermined by Wertheim after the method of C. du Bois-Reymond (*v. Graefe's Arch. f. Ophth.* XXXIII, 2). He made use of a plate of tinfoil pierced with 460 holes of 0.24 mm. in diameter. The distances were found at which the holes seemed to form lines and the lines a surface. Calculations from the first give 74, from the second 147 visual units in the centre of the fovea, to a square 0.01 mm. on the side. Going out from the middle of the fovea toward the temple, the following results were found for the same area:

Distances in mm.	0.	0.15	0.3	0.45	0.6	0.7	1.0	1.55	2.4
From first measurement, .	74	37	27	28	24	18	15	13	10
" second " .	147	77	53	53	50	40	33	27	20

The sudden fall in the number between 0.6 and 1.0 mm. is probably due to passing into the region of more numerous rods outside the yellow spot.

The time required to perceive the form of images that do not extend beyond the yellow spot has been found by Nordmann (abstract in *Jahresb. der Anat. u. Physiol.* Bd. XVI, Abth. 2, p. 176) to be for the adapted eye inversely proportional to the illumination. With constant illumination the time increases inversely as the size of the image. This seems true also of the unadapted eye, but the absolute time is longer. The illumination used in the experiments was furnished by a candle behind ground glass, the time was regulated by a pendulum, the objects were Snellen figures (*Haken*).

It is familiar that faded after-images may be recalled by rubbing the eyes. Dr. Ch. Féré finds (*C. r. de la Soc. de Biol.* July 30, 1887) that the same can be done in other ways, as for example by applying a tuning-fork to the skull. He finds also this somewhat similar phenomenon (*ibid.* Dec. 10, 1887). A sensation too weak to reach consciousness alone may succeed in doing so with the help of a strong one in some other sense. An hysteric looks at letters that are too far away to be read. They are then covered, a strong sensory stimulus administered, and the letters are read from their previous impression. It would be extremely interesting to know whether such experiments are possible with normal persons.

In the opinion of Bjelow (*Westnik. Opht.* IV, 3 and 4; Abstract in *Jahresb. Anat. u. Physiol.* Bd. XVI, Abth. 2), the rest position of the eyes is divergent. The eye, however, does not necessarily return to it when simply covered. Even the introduction of prisms cannot cause the eye to diverge beyond its rest position.

A. Chauveau, after a study of the mechanism of iris movements (*C. r. Soc. Biologie*, April 14, 1888), pronounces for the non-existence of a dilator muscle of the iris, that office being performed by simple elastic fibres. He bases his statement chiefly on the fact that the latency period for both contraction and dilation is the same, about half a second, which could hardly be the case if a *dilator iridis* were really present and innervated through the roundabout way of the cervical cord and cervical sympathetic. In this view he has the support of Rouget, Grühagen, Retterer, and others. The latency period was determined by observation of the diffraction circles of a set of needle-pricks in a card held before one eye while the other was darkened and lighted.

Eye-strain is a recognized factor in the production of many nervous troubles. Dr. Geo. M. Gould points out (*Med. and Surg. Reporter*, Sept. 29, 1888), in addition, an obvious mental and educational effect. The child for whom the process of learning is made laborious or painful by imperfect vision, is so far handicapped, and as a result may appear stupid and actually be disinclined to study. In one of three cases given in illustration, correction of the optical defects seems to have been followed by most happy intellectual results.

A convenient chart for rapidly testing the vision of school children and others is that devised after the principle of Snellen by Prof. Hermann Cohn. The essential part of the chart is a set of thirty-six capital letter E's, arranged in a square, six on a side. Each letter is itself about a third of an inch square. They face up, down, right and left. Any one that can tell their facing at the rate of 1 per second with proper illumination, at a distance of six metres, has the acuteness of vision assumed by Snellen as unity, $\frac{6}{6}$. If one has to come a metre nearer to do so his vision is $\frac{5}{6}$, and so with the rest. The chart is provided with four brass eyes, one at the middle of each side, so that it can be given four positions, and the possibilities of learning the positions by heart made less. A set of lenses in wooden frames for ready handling would also allow the fixing of the degree of short or over-sightedness. When the sight of one pupil has been established as normal, the chart may be used to find whether or not the illumination of the school-room is sufficient, by finding whether he can tell the facings at the normal distance.

In *du Bois-Reymond's Archiv*, No. 3-4, 1888, L. Jakobson proposes the use of the receiving telephone thrown into motion by changes in intensity of a varying current, for the numerical determination of the acuteness of hearing. It may be assumed on physical principles that the variation in the intensity of the sound is proportional to the square of the variations in the current. Gradations of the current are to be introduced by the insertion of shunts of different resistance. While these resistances remain smaller than the rest of the circuit, the variation of the current in the telephone will be, with an error of not more than 2.5 per cent, directly proportional to the resistances used. It is thus easy to so adjust the sound as to find the auditory threshold. The author suggests an apparatus in which a tuning-fork vibrating before the poles of an electro-magnet is used to produce with regularity the original current.

Hernicke (*Untersuchungen über den Temperatursinn*, Inaug. Diss. Bonn, 1887) finds that in tabes dorsalis the discrimination for differences of temperature is much below the normal.

In a case reported by Dr. J. H. Lloyd of Philadelphia, to the last meeting of the Am. Neurol. Assoc. (*Boston Med. and Surg. Jour.* October 11, 1888), it was found, after the arm area of the cortex had been removed for Jacksonian convulsions, that though the perception of form and motion was abolished, the pure sensation of touch was not in the least disturbed.

The self-adjustments of plants with reference to light and gravity have already been the subject of careful study. In the *Sitzungsbr. der Phys.-Medic. Gesellsch. zu Würzburg*, Jahrg. 1888, No. 1, J. Loeb makes a preliminary communication of experiments of his that go toward demonstrating the same in animals. Bilateral animals tend to place themselves so that the rays of light fall parallel to the median plane of their bodies. There is, however, this difference of habit: some turn their oral pole and ventral surface to the light, others their aboral pole and dorsal surface, and in exceptional cases the aboral pole and ventral surface. With equal intensities of illumination, animals seem far more affected by the strongly refracted rays, but the influence of the less refracted rays increases with increased differentiation of organs; so that, for example, flies of *musca vomitoria* were clearly affected by red light, while the eyeless grubs, though under other circumstances heliotropic, were not. The animals seem to adjust themselves rather to the direction in which the rays pierce their tissues, than to light and shade. The intensity of the light also is important, for only within certain limits of intensity are the phenomena to be observed, and the quickness and precision of the adjustments are different for different intensities. That the reaction is really to light and not to heat, is proved by the fact that diffused daylight, light passed through concentrated alum solution, and the more refracted rays of the spectrum, all produce the turning, the last in a greater degree than the hotter rays. In the author's opinion these phenomena are related to "seeing," and indicate that what we are wont to call a psychic function is in some sense a property of all living matter.

In response to gravity, flies with clipped wings set themselves, other things being equal, so that their median plane is vertical and their head is up. In other experiments, roaches very strongly objected to having gravity act perpendicularly upon their ventral surface.

In somewhat the same direction, but upon lower organisms, were the experiments of R. Aderhold (*Jena'sche Zeitschr. f. Naturwissenschaft*, XXII, 1–2, 1888). He found that Euglena and others are not influenced in their direction of movement by water currents, *i. e.* are not rheotropic; they are, however, aërotropic and geotaxic. The last two characteristics would be of use to Euglena in coming to the surface when accidentally covered up in any way. Other organisms, including diatomes and oscillaria, seem to regard none of these things and respond to light alone. Desmids are positively phototaxic with weak light and negatively with strong light. Algae set themselves with a definite position of their axis toward the light because they tend to move toward it. The angle at which they place themselves with reference to the surface below them in moving forward seems always to be that at which friction is least.

Two peculiar cases of aphasia have been observed by Dor (*Rev. général d'Opht.* 1887, p. 155) in which, while color perception remained, the color names were lost.

In a study of the mental state of the hereditarily degenerate who do not reach full insanity (*Arch. génér. de méd.* March–April, 1888), Ballet relates, with other cases, one of a bookbinder, of convergent bad heredity, 37 years old, and a polymath in a small way. After a long period of daily attacks of palpitation, shortness of breath, aura-sensations, etc., he experienced while reading an attack of a severer sort, with loss of consciousness. After one repetition this gave place to agoraphobia. For fear of other attacks he stopped reading, and even the sight of a book or an unfamiliar word threw him into mental anguish, palpitations, and perspiration. Still later, on reading a word from a placard he felt suddenly uneasy, had to repeat the word several times, but for all that, forgot it except one syllable; then that seemed to rise up to his brain and he fell down unconscious. Not till the next day did he recall what went before the fit. After a second such attack he attempted suicide, and developed slight delusions; and after a third, became paralysed in the left arm and leg. He had a contracture on the right side of the face, complete anaesthesia on the left, and a hysterogenic point in the lumbar region, and showed transfer phenomena with a magnet. His memory and intelligence suffered, and he was sensitive and irritable. He showed a misshapen skull, sexual impotence, *folie de doute* and arithmomania. Attempts at reading ended in unconsciousness, or he stopped at some word and declared that his tongue forbade him to say it; when at other times he read past it, he saw it advance along the line and his field of view became dark. The loss of consciousness after the "onomatomaniac" seizures, Ballet attributes to an hysterical attack induced by them, and not to the seizures themselves.

Dr. Frigerio reports a case of persistent refusal of food (*Archiv italian. per le malat. nervos. ecc.* 1888, XXV, p. 98), on the part of a syphilitic paranoiac. Though willing to make repeated trials, he saw his food each time magnified to such an extraordinary heap that he was turned back. Anti-syphilitic treatment produced improvement in other respects but not in this, nor was the illusion destroyed by excluding accommodation with atropine. Ophthalmoscopic examination only showed signs of syphilitic retinitis.

In a paper presented before the psychological section of the British Med. Assoc. (in abstract in *Brit. Med. Journal*, September 1, 1888, p. 484), Dr. Oscar T. Woods gave this interesting case of *folie à quatre*. Four members of a family, mother, son, and two daughters, were infected by a third older daughter, and to that degree that a child of the family was murdered by the mother. They were found violent and partly naked, with the delusion "that having killed the fairy, they were freed from their sins and went to heaven." The four recovered in a couple of weeks; the daughter by whom they were infected remained insane.

Nervous children are frequently on the verge of being overpowered by their own imaginations, and offer a ready soil for the growth of hallucinations. Moreau, in an article on the hallucinations of children (*L'Encéphale*, No. 2, 1888), collects ten cases, mostly from recent literature, of children between the ages of two and fifteen thus affected. The exciting causes fall into two classes: moral causes—for example, horrible stories, excessive religious training, life with others subject to such disorders, the terrors of the night, over-vivid impressions of some kinds; and physical causes, like fevers, poison, and disorders of digestion. Hallucinations of sight and hearing are by far the most common, and they are almost always sad and terrifying—devils, monsters, spirits, witches, threatening men, and the like.

Under the title of "*Ein genesener Paralytiker*," Schaefer gives, in his Inaugural Dissertation (Berlin, 1887), some references to the literature of recoveries from general paresis, describing in detail the case of a man who recovered under his care. The recovery is, in all cases, consequent on severe suppuration.

The *Journal of Mental Science* for October 1887 and April and July 1888, contains a long and interesting review, from the standpoint of the alienist, of Dowden's Life of Percy Bysshe Shelley. The poet is shown wavering along in the region between sanity and insanity, sometimes upon one side, sometimes on the other. He was of bad nervous heredity, undersized in brain, unstable, very susceptible to the other sex, had hallucinations and delusions, and yet withal a genius of a type that critics have not scrupled to call angelic.

A stunted brain is not a guarantee against disease of such mind as accompanies it. Dr. Henry M. Hurd (*Am. Jour. of Insanity*, Oct. 1888) shows, with illustrative cases, that insanity is to be found among imbeciles above the lowest grade. The latter are indeed only irritable and impulsive, but those a little higher are perverted, and their impulsive acts are those of insanity and may reach suicide and killing. They do not, however, have delusions and their mental disturbances are transient. In the highest class there are manias and melancholias, simple or with delusions, and their course is not different from that in other insane. Pianetta reported to the congress of Italian physicians at Pavia (*Riv. speriment. di Freniatria*, XIII, 1888) observations on 114 imbeciles in the asylum at Imola. 51 were demented and quiet, 44 demented but occasionally excited and violent, and 19 showed clear mental disease on a foundation of imbecility. Of these 19, ten were maniacal, eight were melancholiacs, and one a paranoiac. The degree of imbecility in these cases was slight, but the road is short to worse conditions.

For the purpose of comparison with similar observations made by others on the disturbances of reading in cases of progressive paralysis, F. Kraemer has studied the loud reading of the uneducated, the aged, and the non-paralytic insane (*Verhandl. d. Physik.-Med. Gesellschaft zu Würzburg*, XXII, 4.) The errors that he found were for the most part insignificant, except for a few of the aged, chiefly those in whom senile dementia had already begun. They, like the paralytics, distorted many words, and added others that had no similarity in sound or sense to those in the text. The non-paralytic insane, even where their alienation was extreme and of long standing, read for the most part with as few errors as the sane.

In his oration on "Education in its relation to Insanity," before the alumni of Haverford College, 1887, Dr. Robert H. Chase furnishes thought for both optimists and pessimists in educational matters. He emphasizes the fact, already known, that on the one hand, improper and unhygienic education is a fruitful source of insanity, and on the other, that the bulk of the insane come from among the uneducated, and that proper education is one of the surest safeguards against its increase.

Cionini (*vide* report of Italian medical congress at Pavia, *Riv. speriment. di Freniatria*, XIII, 1888) measured the thickness of the cortex in 15 cases of paralysis, making 150 measurements on each, 50 in the portion forward of the rolandic region, 50 in that region, and 50 in the portion behind it. He found a general thinning of the whole cortex, most marked in the posterior central convolution, less so in the anterior, still less in the parts further forward, and least in the rearward parts. It is less marked in the left hemisphere than in the right. Other things being equal, it is thinner in the depths of the sulci, and on the basal and median surfaces, than on the convolutions and convex surface.

A. Borgherini reports, in the *Riv. speriment. di Freniatria*, XIII, 4, the examination of the brain of a dog that had shown clear symptoms of ataxy. The cerebellum was only slightly small, but showed microscopically extended atrophy, irregularly scattered over its cortex, and especially marked in the worm.

Exner and Paneth (*Pflüger's Archiv*, Vol. 40), in the course of certain other experiments, extirpated the gyrus sigmoideus on one side in six dogs. In five of them, disturbances of sight took place which lasted for about four weeks. In six other cases, where the injury was of slightly greater extent, the same "crossed sight disturbances" were occasioned—injury on the right side caused nearly total blindness in the left eye, on the left side in the right eye. Recovery took place in from 7 to 37 days.

A somewhat full discussion of Brown-Sequard's hemilateral lesion of the spinal cord is given by Nolte (*Brown-Sequard'sche Halbseitenläsion des Rückenmarks*, Inaug. Diss., Bonn, 1887). The body of the dissertation is taken up with the description of a man in whom a knife-wound between the second and third dorsal vertebrae caused a hemisection of the cord and a clear exhibition of the corresponding symptoms. The author does not contribute at all to the anatomy of the subject, but the clinical literature is largely given.

Frigerio endorses Ferrier's location of the centre for smell in the *cornu ammonis* (*vide* report of Italian medical congress at Pavia, *Riv. speriment. di Freniatria*, XIII, 1888), on the ground of a case of traumatic paranoia, with hallucinations of smell, in addition to tactual and auditory illusions. The autopsy showed extreme atrophy of that region on the left side.

The ideal way in which to instruct students in the gross anatomy of the central nervous system, is by means of fresh material and elbow advice, both in unlimited quantities; but when students are counted by the hundreds, and the time is very limited, this plan must be modified for the good of the greatest number. No course is better, perhaps, under these conditions, than modelling the parts of the brain on an heroic scale before the class. Prof. John Curtis, of the College of Physicians and Surgeons of New York, has made extensive use of this means, and his models in clay, photographs of which have been made by Dr. Warren P. Lombard, show how admirably some of the difficult points in brain anatomy can be thus elucidated.

W. P. Herringham, M. B., gives in *Brain*, July, 1888, an interesting note on heredity, under the title of "Muscular Atrophy of the Peroneal Type Affecting many Members of a Family." Members of five generations are listed. The strange feature of the inheritance, and one that the author says is peculiar to the family and not the disease, is the frequency with which perfectly healthy daughters of the line have transmitted their fathers' infirmity to their sons but not their daughters. No daughter of the line is reported as diseased.

The following method of studying cerebral localization in a way different from those of Hitzig and Ferrier, is privately suggested by Dr. Clevenger. He says: "If peripheral points all over the body be stimulated electrically, successively, and the bared brain of the animal, either anæsthetized or recently killed, be covered with paper saturated with the ferro-cyanide of potassium liquid, such as was used in Bain's telegraphic recorder, through which paper the circuit may be completed by a checker-board arrangement of many small metallic plates, it seems probable that the least resistance channels will be indicated by the blue discoloration on the paper at points corresponding very likely with the centres for the stimulated peripheral points. Galvanometric deflections could also be observed. Diffusion would surely be prevented by thus affording many separate points of escape for the current. As to whether it would travel in the course of nerve strands remains to be seen, but with proper precautions I cannot see why it should not do so."

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EXTRACTS FROM THE AUTOBIOGRAPHY OF A PARANOIAC.[1]

Edited, with a Commentary, by

FREDERICK PETERSON, M.D.

As a preface to the autobiographical sketch of this interesting religious paranoiac, I make a few transcripts from the medical records of the Hudson River State Hospital for the Insane, of which institution he was an inmate for over seven years, from July 29, 1872, to October 11, 1879. He was thirty years of age at the time of admission, single, and a farm laborer by occupation. He was not a church member, had a common school education, and was a native of the United States. Hereditary predisposition was not acknowledged. His mother, who accompanied him to the hospital, stated that he had always been delicate in his physical constitution and given to despondency. Since the age of twenty he had done little or nothing, because of ill health. A year previous to his commitment as a lunatic to the hospital, he shot himself in the forehead, in an ineffectual attempt at suicide. Later he developed delusions that the people of the village were acting upon him by magnetism, spoke disparagingly of him, and were conspirators against his peace. During the whole of his sojourn in the hospital he had hallucinations of hearing, and in the earlier period of his stay, delusions of persecution. Toward the end of his seven years of hospital life he gradually developed, in addition, delusions of grandeur. Although he had occasional lapses of self-control, manifested rarely by the breaking of window glass or the tearing of clothing, he was for the greater portion of the time sufficiently self-possessed to restrain whatever violent or destructive inclinations he may have had, and was permitted to go out alone upon the large grounds of the asylum whenever he wished and to wander about the woods at will.

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[1] Read before the Neurological Section of the New York Academy of Medicine, Dec. 14, 1888.

It was during the years 1878 and 1879, the last two of his stay at Poughkeepsie, while still the victim of constant auditory hallucinations and of mingled delusions of persecution, unseen agency and grandeur, that he wrote an autobiographic volume of four hundred manuscript pages, with the extraordinary title-page presented below :[1]

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[1] It was first written in phonetic cipher, on pieces of brown wrapping paper, but subsequently copied in a neat hand on note paper and bound by himself in a 400-page volume. A New York publisher once had the volume in press, but the forms were destroyed and the publication suppressed by relatives of the patient.

THE

PILING OF TOPHET

AND THE

TRESPASS-OFFERING:

A

TRUE LIFE HISTORY.

BY

F. T. J——,

Patient in the
Hudson River State Hospital for the Insane,
Poughkeepsie, N. Y.

> "For Tophet is ordained of old; yea, for the king it is prepared: he hath made it deep and large; the pile thereof is fire and much wood; the breath of the Lord, like a stream of brimstone, doth kindle it."—Isaiah xxx. 33.

The following is the dedicatory page:

To
Sinners,
who in every sin against their neighbor, by word or manner or responsive feeling, have, from the foundation of the world, been laying their own share of fuel on this unlighted pile;
and to
Saints,
whose acts towards their God and towards the world have been but an instinctive prelude to this supreme sacrifice,
in the name of
Nature
and its unfailing laws,
and of its
Founder
and His Eternal Kingdom,
this book
is
worshipfully
dedicated.

The book itself is a deeper history of his life and mental evolution than any but himself could furnish. It is remarkable for its excellent literary style, and for its keen reasoning and psychological analysis of his own disordered mind. In it he dissects his hallucinations and delusions like a skilled anatomist. It is as fascinating as a novel. I regret that I can give but extracts here and there which have an especial bearing upon the elucidation of his psychosis.

In the Preface he defines the scope of the book as follows:

> "This work is given to the public as a lunatic's defense of his position. Every effort I have made hitherto to come to an understanding with my fellow-men, on things which I see to proceed from them, and which give my life its whole shape, has drawn out nothing more than blank denials of all knowledge of the things I spoke of. Now it is impossible for me to reduce my thought to the bounds which others have been willing to concede. The object of this little autobiography is to show the form and consistency of the thought that is in my mind.
>
> "I present my evidence to the tribunals of last resort, the public and the press, and ask them to try the case and render their verdict. Have I a right to my thought or have I not? If not, where am I deceived? If I have, why is not mine the *true* thought for all men?"

A paragraph from the Introduction further reveals the object of his confessions:

> "A person is supposed to have a reason for what he does, and I might consider it incumbent upon me to tell the motives which actuate me in thus entering upon the work of the scribe under circumstances so peculiar. Is there anything I have to tell that might not as well and more safely be left untold? It is a question which I do not have to consider and decide to-day, for I have been long inspired with the conviction, the consciousness, that I *have* something to tell that it would be worth the world's while to hear."

In another introductory paragraph he makes an excellent diagnosis of his mental infirmity. Addressing his reader he says:

> "I did not tell you that I am a patient in an asylum. I am to take it for granted at the outset, that my prospective reader knows nothing of my character, condition, or circumstances, beyond what I tell him. I am here as an insane patient. I have been here over five years. . . . . Being an insane man, it will be nothing unexpected that I should, in giving these reports of my fortunes, narrate incidents and particulars partaking more or less of the marvelous or preternatural. I am not only a lunatic, but one of the class of lunatics having a controversy with the world in general; in other words, possessed with a monomania, or crazy one-sidedly, or on a single subject."

In the hospital record presented above, nothing is adduced as to heredity in his case, and but little stated concerning his mental condition in early youth. These deficiencies are, to a great extent, supplied in the autobiography. I shall permit our author first to describe his appearance in this world, in a cyanotic condition, and the characteristics of his childhood and early youth, and subsequently the hereditary influence in his destiny:

> "It is said that I was entirely black when I was ushered into the world, and that for I forget how long a period of time I did nothing but give vent to heart-saddening wails. Was I lamenting the gift of light, on this morning of what was to become a woe-burdened existence?
>
> "I was a weakly infant. I came near dying of the whooping cough, and it was always asserted by those who knew, that I owed my life to the untiring exertions of a poor woman who lived a neighbor, who busied herself all night with me, dipping me at intervals into a tub of warm water. My half-sister had it at the same time and died.

"It will be of use to give an idea of my nature and disposition in my tender years. I was always a shy, retiring child, not disposed to make myself free with strangers, not much given to prattle—in fact, one of the sad and silent sort from the first. I can remember some peculiar sensations which used to weigh on my mind, which go to show that the foundation of my mind-life was but imperfect from the first. I used to be troubled with very strange feelings when I was waking out of sleep, especially if I had been taking a nap in the daytime. It used to seem to me that I was floating in the air, and I often thought to myself: 'Why, how queer I have been feeling!' It was as if I filled the whole room, way up to the ceiling. I was told by others that I sometimes raised myself up in bed after getting to sleep and made an outcry, 'O don't, O don't!' seeming to be in great distress; but the strange part of it is that I could remember nothing about it. I do not think that I ever remembered even their waking me, or finding them at my bedside. I only had their word for it next day.

"As far as I can go back, I remember having at times, but not frequently, impressions which must be identical with what I have lately heard others speak of as 'double memory.' The feeling would all at once creep over me that the very thing I was present with, my ideas and perceptions at that time, had happened to me once before in just the same sequence and arrangement. I have heard this explained as due to a lack of simultaneity in the action of the two lobes of the brain, the tardy one remembering what had already passed through the other. My own theory was different, leaving the organ acting out of consideration. I only went so far as to look at it as a mistaken quality in the perception—an erroneous attaching of the nature of the act of remembering to what was really the act of thinking in the present.

"I was very early in life an observer of my own mental peculiarities, to a degree which I think must be a very rare exception. I often used to be sensible of an unsatisfactoriness in my consciousness of what surrounded me. I used to ask myself, 'Why is it that while I see and hear and feel everything perfectly, it nevertheless does not seem real to me? It is as if I were in danger of forgetting myself and the place where I am!' I often wondered even how I kept the run of things as well as I did. I always found myself holding on to the orderly and proper connection of my acts, and yet from my feelings I could not have answered for my doing so. I can remember sitting at my desk in school, when a small boy, and dwelling with melancholy on this dimness in my perception of existence, and wondering how it was with others in this respect. I wondered to myself if life, as ordinarily bestowed, included this deficiency.

"I showed in my tastes and behavior a harmony with the internal composition of my mind. I was never given to the active sports which the common run of boys take so much delight in.

"The simple fact is that I had a languid nervous development, and from the necessity of my organization could not have much capacity or relish for sports of agility.

"If I could compound a boy of my own I should try to improve on the model I remember to have exhibited in myself.

"It is not true that I was regarded or treated as strange or deficient in my wits. Such an idea would look misplaced to those who knew me and consorted with me in those days. These differences are perhaps more evident to myself than they ever were to the greater part of my acquaintances. I brooded on this side of my character at a later period, and I no doubt remain liable to give greater prominence to disparaging traits than some impartial observers would justify me in doing.

"As a general rule, my harmless and peaceable disposition kept me out of squabbles with my schoolmates. If I was approached in an aggressive way I met it with absolute non-resistance, which in my case had the disarming effect which is attributed to it by pious moralists."

"If we change the scene from the playground to the schoolroom we shall find that I attained a distinction of my own apart from the average, and more to my advantage there. I was always a favorite with my teachers. I never gave them any trouble, and took to my studies with a willing relish that could not but be pleasing to them. I learned to read before I went to school; in fact, like an old asylum acquaintance, Mr. M., inventor and infidel monomaniac, I can almost say that I can't remember when I could not read.

"I was frequently singled out for complimentary remarks on my proficiency in my studies. I gave evidence of some talents of a higher kind—could draw, for instance, better than any boy in the school.

"One of the most marked weaknesses of my character, as a child, was my susceptibility to being teased.

"After having pondered some on the traits of the human animal in this particular, I have come to the conclusion that there is no further explanation needed than that the impression made on the teaser by the teasable is such as to naturally prompt the acts constituting the teasing, as the sense of burning makes us shrink, and an aroma suggestive of a fine flavor tempts us to bite. I feel convinced that the liability to be teased rests on a principle that has a mighty influence in the motions of the soul of humanity.

"My misdeeds, as a child, were rarely prompted by a love of mischief or the result of headlong thoughtlessness.

"I had a well-defined idea of the nature of sin, and I used frequently at night to recall the events of the day and reflect on instances in which I had transgressed and given way to ill-humor, and form resolutions to try and do better. From some of the most flagrant of the sins and improprieties to which small and larger boys are prone, I was entirely free."

Our patient, as has already been stated, was not a church member, and in his book he describes his early religious life and his subsequent beliefs as they developed. His father was a Universalist and his mother a non-professor of religion, although she did attend the Methodist Church. During his boyhood he attended Sunday School regularly, and at one time the Episcopal Church. But his attendance upon divine service ceased in early youth. Both parents were honest, conscientious, and highly respected in the community. They were first cousins. The mother was healthy in mind and body, but the father is reported to have been exceedingly eccentric, possibly insane. From what I have recently learned regarding him, he also was something of a paranoiac. They strove to bring up their children carefully and educate them as well as possible. Concerning their inculcations in his youth, he says :

> "My early training cannot be said to have been a predominantly religious one. My mind was neither imbued with ineradicable prejudices, nor prepared for reaction to the other extreme by excessively rigid sectarian drilling and formalism."

His father died when he was twelve years of age. Up to the age of thirteen he attended a country school, both winter and summer, but after that his farm-work permitted him only winter schooling. Still, he evidently had unusual talents and aptitudes, and we find him later studying by himself, in the original, many of the classic Latin authors; and among his favorite companions were the works of Boethius, Lucretius, and Josephus and the Bible. His literary style and modes of thought are in themselves an evidence of more than ordinary attainments in rhetoric, philosophy, and logic. I regret that I cannot present this book in full, because every page has its value as an index of the condition of his mind from childhood to the last years of his confinement in the asylum, and the story is told with a directness and simplicity that marks truth upon every statement and lends it such charm as pertains to all works which portray life with the utmost fidelity. In its way it has as much value as Amiel's *Journal Intime*, and its psychological analysis ranks higher, in my estimation, than some of the artificial work of Dostojewsky (Raskolnikoff).

The matter of heredity in his case was not sifted thoroughly upon his admission to the hospital, nor have I since been able to gather much material relative to this factor in his evolution. But one important element of this nature is described in his book—an element not only hereditary in its character, but for a long time part of his environment, and undoubtedly an influence modifying his mental condition both before and after his birth. I allude to a great-uncle, a brother of his grandmother on his mother's side, who was himself a paranoiac, and who lived upon the farm in intimate companionship with our patient until the latter was twenty-three years old. I cannot do better than permit the author to describe this peculiar personage and their life together, in his own words:

> "I worked steadily upon the farm, though with moderation, at such kinds of work as I seemed to be equal to. The heavier kinds of work, such as plowing and wagoning, as also the marketing of the produce, were attended to by my great-uncle.
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> "It is a somewhat delicate subject to manage to my satisfaction, this that I am about to enter upon, but it demands candid and impartial treatment, because the events that followed in later years cannot be rightly understood without it. It is impossible for me to give a veracious sketch of my soul-life during this period without dwelling quite minutely on the characteristics of my great-uncle. He was a man who had roughed it a good deal in the world, had been at one time in his life a live-oaker in Florida. How his temper and disposition may have been at an earlier period I cannot say—I only remember him as a man possessed of the belief that a certain young man living on an adjoining farm had the power to torture him at his pleasure, both by bothering his brains and inflicting physical pain; which power he made use of to such good effect that the poor victim was almost constantly kept busy holding him at bay by means of cursings of the most fierce and vigorous description.

While at work with the horses in the fields, and when driving, he would intermix his commands to the animals with savage execrations of the troubler of his peace. The unfortunate man was troubled, at certain seasons of the year especially, with sore feet, and at such times his imprecations against the offender would fairly rise to yells, and were almost blood-curdling in their intense ferocity. Thus it went on day and night. He slept in a small room in one of the outbuildings, and often he could be heard a great distance off shouting out threats, sometimes throwing boots or boot-jacks against the boarded side of the building where he lodged, to put in the interjection points.

"It may be imagined that a boy of a reserved and sensitive disposition, as I was, could not assimilate very well with such a character as this. I was always distant in my intercourse with him, and a feeling of aversion for his habits of savagery led me to avoid coming in contact with him more than was rendered necessary by our joint labors on the farm.

"As the years passed on and I continued to live in the presence of my uncle's fierce demonstrations of hostility against the invisible destroyer of his comfort, my tolerance for his conduct insensibly gave way. I had now reached the age of eighteen or nineteen, was a tall slender youth, not strong either in nerve or muscle.

"The exhibitions of his ruling passion called up more and more determined feelings of antagonism in my breast.

"Before I knew it I had gone a criminal length in my resentful feeling. I came at last to feel that a person of such a thoroughly savage character did not deserve more indulgence than a mad dog. My position from that time was one of contingent murder. Alas! that I should have been content to let such a state of things last a single day. The frightful danger of my situation ought to have been sufficient to spur me to sacrifice everything to escape from it. But I was in chains, the chains of apathy, impotence and incapacity, and I could only stay where I was and fume against the object of my detestation.

"I must always regard it as one of the most unfortunate things in my unfortunate career that I should have been placed in contact with this much to be commiserated sufferer at such a time of life. It was not the man himself that I hated. When my judgment could act without impediment, I saw that his unpleasant behavior was entirely the phenomena presented by his never-ending war against what was in his eyes the most wicked and cruel of persecutions. I could then pity him and dismiss all rancorous thoughts."

This antipathy led to a change in the residence of our author. He felt that he must be separated from his uncle, and accordingly he removed to a town at some distance from the farm. It is curious that he never speaks of his uncle as insane, and it is probable that both his mother and himself and other relatives regarded his persecutory delusions as merely evidence of eccentricity. Soon after removing to town he had some pulmonary difficulty, and he speaks at some length of this as follows:

"In the depressed state of my nerves I imagined myself much worse than I really was, and like many others in the same condition, I felt as if I was liable to sink away and die at any time. My disease was accompanied with periodical accesses of fever, and in the fictitious strength of excitement given by this, my mind seemed to gain an abnormal activity. It was at this time that I first received a revelation on the mysteries of the human soul that had an all-dominant effect on my destinies and the turn of my thoughts ever after. . . . I now learned what had always been to me a hidden mystery—what was the meaning of strength of will and strength of intellect. Before I had ever lived enshrouded in mists and clouds. In that transitory strength given by the fever coursing through my veins, I now saw the man I ought to have become rising up like a shadowy phantom in judgment on the wreck which I really was. . . . My agitation was so great that my mother and the neighbors seemed to fear I was going crazy. *I* felt that I *had* been crazy for a long while and had just recovered reason. It was a fact. But I was constrained to lock up my remorseful agony in my own breast."

We have seen that our patient was throughout his early youth morbidly subjective, and his hypochondriasis increased with years. He had now attained the age of twenty-three; we shall let him describe his mental condition and habits of life at this time. In this description we shall see the gradual growth of persecutory ideas upon a favorable soil:

"My strength and endurance were not sufficient for manual labor, and I did not feel confidence enough in the clearness and energy of my mind to justify me in making application for any post where head-work would have been demanded, or for which ready presence of mind, or a good address, would have been required. But it was the unpleasantness felt on contact with my fellow-men that operated more strongly than anything else in binding me down to the course of life to which I devoted myself. I felt my deficiencies most keenly every time I met a human being face to face. . . . I could not do otherwise than shun what was so galling to my sensibility, while appearing to conduce to no desirable end. . . . . But I am going to show that I still remained exposed to very great dangers, and it is as true as it was before that I shunned the only means of averting the calamities threatening me, no doubt of necessity at this stage, and in obedience to the eternal decree that every tree shall spread out and develop in accordance with the qualities given to it 'before it was in the ground.' I did not like the constraint imposed upon me by the presence of man. I did like the freedom of solitude. I strongly disliked many things I noticed in the manner and words of some I met, and there was nothing to prevent this dislike from occasionally being absorbed into my solitary musings, to find its final resolution in the passion of indignation in its various degrees of intensity as the case might be. I have spoken before of my defective means of defense against 'teasing' or mocking for the purpose of troubling. I was always terribly alert and sensitive to all kinds of 'snubs' and sneers, and oblique remarks in general, on their proficiency in which some people pride themselves so much. . . . . I was also disagreeably impressed by the ways of some who showed a disposition to turn their attention to myself, instead of confining themselves to the subject I was presenting to them.

"I was being carried into a state of secret enmity to mankind in general by the prevailing tenor of my brooding meditations, and there was no corrective present.

"But all received a hue from a yearning for what was worthy in life, paired with a mournful sense of its hopeless absence. Whatever wrong turns I may in my weakness have been betrayed into, it is impossible that I should look upon my then existing frame of mind as a whole with repentant feelings. As well condemn righteousness and holiness itself!

"When I admit that I occasionally was overcome with an irruption of hard feelings toward wrong-doing man, it will of course not be understood that I was habitually morose and spiteful in temper. Nothing could be further from the truth. What commotion there was, was mostly internal, rarely reaching the surface in visible ebullitions. . . . I occupied myself with the trifling labors of my garden, dwelling with interest and pleasure on the progress of my crops and flowers, and every now and then took a ramble over to the woods lying to the south, which were a favorite place of resort to me all the while I lived there. There I botanized and moralized, explored the recesses of the woods, enjoyed the calm quiet of nature, and groaned over my hapless condition, wondering what it was to come to.

"There were some little things that happened to me the first year after I left the farm which became as it were a kind of sample of what I must continue to expect, and the memory of which had more influence over my action in aftertime than I was aware of myself, no doubt. . . . When I was around the city, thinking I might get employment I called on one of my old acquaintances, who was then in a store. I talked with him a few minutes at that time. I called again a short time after, when I was told by the proprietor that the gentleman I had called to see was not in. There were a number of men present in the store, salesmen, and it became apparent to me that they were trying to exhibit an offensive demeanor toward me, or perhaps it would be as true to say that they were moved to make a derisive demonstration against me. At all events, all, with perhaps the exception of the proprietor, stood with contortions of countenance, which was perhaps laughter, until I retired. . . . I found it hard to consign this to forgetfulness. At first it lay dormant, but it would come up, and I must confess I had hard feelings, even revengeful feelings, toward the actors. Another thing happened the same fall. I went to a store and standing at the counter was noticed by one of the clerks, an Irishman, who came to me and said, 'I always wait on the little boys first,' and as I took no notice of the remark, seemed so determined his words should not be lost on me that he repeated them with the addition, '*like you*.' As before it produced no immediate effect, but it afterwards rose and rankled in my memory, and I was not able to keep clear of imagining vindictive things. In fact, to tell the truth, in both cases I felt that blood would have been sweet to me. . . . My mode of thinking on these incidents no doubt had in it much of the character of insanity. . . . The effect was that I got settled down into the fixed idea that contact with the thoughtless evil world, in my state of body and mind, would impose upon me the necessity of committing crime in vindication of my honor. . . . I let these bloody memories tinge my whole mind and all its anticipations and resolutions for the future. . . . 'I see,' I said to myself in substance, 'that these galling collisions are the natural penalties of being imperfect.'

"It may be as well, for the prevention of misconceptions, to say that I never took one step toward putting any design thence arising into execution. I had no designs. I never armed myself, or, in fact, went any further than to rehearse the drama of revenge in my own mind. The pistol I bought was one which I would not have trusted for a moment to carry for the purpose of self-defense. . . . . Nevertheless, the events on the farm show that my wickedness was not altogether of a mimic kind, and I will not attempt to escape righteous judgment.

"I used to make many resolutions about regularity in habits of eating which I found myself powerless to keep. A sense of depression and vacuity would come over me, aggravated by my solitary, monotonous life, I presume, and often by an obstructed state of the alimentary organs. . . . . It is a common feature in insanity or semi-insanity left to itself, I think. I also exerted my brain to the extent of abuse, I know, in the way of study. . . . . I used to study Latin for a pastime, and often kept cudgeling my brains over Cicero and Cæsar until the top of my head was very sore. This solitary immersing of an enfeebled mind in study, with obliviousness to myself and all surroundings, was, no doubt, a help toward the grand consummation that took place in the fulness of things. . . . . I suffered a good deal from bodily ailments. My liver seemed to be thoroughly out of order and torpid. I had a feeling of hardness and inflammation in my sides regularly, a certain length of time after meals; digestion was bad, appetite irregular—in fact, every sign of a deadlock in the vital functions."

We have seen in these excerpts from the author's account of himself how heredity and environment had gradually moulded his physical and mental characters. A shy, timid, delicate child, clever intellectually, given to oddities of speech and conduct, inclined to solitary musing, rarely sharing the sports or games of other boys—in him were slowly evolving marked eccentricity of demeanor, a disposition to shun his fellows, a misinterpretation of their looks and actions as regarded himself, a morbid egotism, a consciousness of a gulf between himself and ordinary men, with deep depression, outbursts of passion, an inclination to homicide restrained but feebly by his weakened will, and delusions of persecution. No doubt the derogatory remarks he fancied expressed about him in the stores were the first harbingers of auditory hallucinations. Of late he had murder in his thoughts, through the morbid humiliation he felt at the imaginary insults from others. No doubt, as his conduct grew more and more strange, he did attract attention among his fellow-men, and this, unfortunately, would but feed the flame of his pathological self-consciousness.

His mother and himself removed to another village in 1871, when he was 28 years of age, by which time there was but little question of his insanity, even among his relatives. I let him take the thread of the story again at this epoch:

"When my mother was making preparations for moving she asked me to help in packing up some chairs. I made an effort to apply myself to the task, but suddenly found myself overcome by my feelings, and before I knew what I was about I had shivered one of the chairs to fragments. A most unpromising omen! The fact is that I was and had been for some time in a state which any physician, knowing the facts, would have pronounced to be unmistakable insanity. But I had different ideas about what constituted insanity, and often thought to myself that if I did get put into an asylum, as had been threatened, they would not keep me, because they would see I was perfectly rational. I have learned more about the subject since.

"Things of the kind I have told of had happened to me before, at uncertain intervals, during several years, an obstructed state of the bowels bringing on a turn. I would get into such a condition of exaggerated discomfort as to lose for a moment, or sometimes quite a spell, my control over my actions and act very strangely. Sometimes I dashed down an article I happened to have in my hands, or demolished the first thing that came to hand; sometimes I gave vent to my feelings by grating my teeth, 'clawing' my face, and going through strange grimaces and agonizing contortions. My face seemed to me to be paralyzed when I had such turns, as if lifeless. The worst thing I ever did was when I flew at my mother in a sudden access of frenzy one day, when she had wrought upon my feelings by talking to me irritatingly, and bit out a mouthful of her hair. . . . . When I was committed to the asylum, at a later day, it was reported as one of my symptoms that I had delusions about my mother being my enemy, etc., but nothing could be further from the truth. . . . . I often grieved in secret over my inability to be a stay and protection to her, bereft as she was of all other support, but all in vain.

"In my new home I was in one of a row of houses, with strangers living near on both sides, and the sense of the presence of the evil which I had shrunk from so long weighed down upon me with crushing weight. After awhile my spell of hypochondriacal despondency passed off, and I settled down into the way of living which I adhered to as long as I remained there. As to getting acquainted with my neighbors, or having any intercourse or dealings with them, that was altogether out of the question. . . I now had more of the feeling of constraint, from the knowledge that I was moving under the eyes of people who were stranger to me than the strangest of the strange could be to a person of the ordinary stamp. Sometimes I heard remarks which did not affect my feelings flatteringly, but that was not common.

"Along in June I had a worse spell than common of the kind of nervous stagnation or will-impotence of which I have spoken, and perpetrated some quite irregular acts before my fetters became slackened. In my despair I tore the collar from my shirt, tore the slippers I was wearing, dashed my fist into a tempting dish which my mother was offering me to eat, and other things of the kind. The house we occupied was owned by a maiden lady who lived with her sister in part of the house. . . . . In the evening after the other sister returned, who had been absent during the day, I overheard a few words which showed plainly enough that the events of the day were being discussed in no very gratified humor. It was evident that my acts were severely reprobated."

The next day the justice of the peace called upon him and admonished him to restrain himself, hinting of the asylum. Of this our author says:

"The dragon's-tooth of reprimand that had been left in my mind grew into a monster, in whose presence I found it impossible to live, and I had a fresh access of despair. It was a hot June morning. I remember seizing a razor and flourishing it, and saying, 'Show me that rascal and I will slaughter him,' or words to that effect, meaning, of course, the justice of the peace."

Both homicidal and suicidal inclinations had long been haunting the secret corners of his mind, for three years before he tells of buying a pistol for the express purpose of making way with himself or some one else. On this day, after meeting the officer, he determined upon suicide. He walked out to two different country stores and bought ammunition. On his way back he passed some men in a field. They all looked at him, and one of them "laughed loud and mockingly, and then cried out in a sort of squealing way, the intention of which could not be mistaken." Then he played a game of croquet with a young man at his uncle's, and overheard the young man make a covert and derisive remark. He continues:

"I passed the next day in brooding, silent, melancholy. It was a rainy day and in accord with my feelings. . . . . That night I wrote a little statement to be left behind. . . . . It cannot be said that I plunged thoughtlessly into the gulf of self-murder. I had from the first gauged the responsibility I was taking on myself, as fully as my mind was capable of doing it. I felt the whole weight of the condemnation that rested upon me for committing such a deed. . . . I passed some part of the hours of the night in sleep. In the morning my mother came to the door to see how I was, and I grasped her hand with a gesture of agonized despair. She took it as an indication that I was going to have one of my wild spells again, and, as she told me afterwards, began to anticipate some work of demolition after I should come down stairs. After she had gone down, I went and took the pistol from the stand-drawer, put on a fresh cap, got into bed again and propped up my head on the pillows, placed the muzzle of the pistol against the center of my forehead, and fired."

He lost considerable blood from the scalp-wound, but the bullet had glanced off; and, although he now tried to starve himself, he was up and about in a few days as usual, attending to his garden with bandaged forehead. He continues:

"There were some steps taken toward getting me into an asylum after my abortive attempt at suicide, but as there were difficulties about it, and I appeared perfectly sensible and rational, my relatives concluded to let it rest.

"From the time of my shooting until the next spring there was not much that deserves mention. How were my thoughts about suicide? It must be said that I had not totally renounced that idea. . . . . I used very often to scan the beams in the wood-house and the coils of clothes-line in the garret . . . . The old difficulty of giving way under the slighting or displeasing demonstrations from others remained as bad as ever. I remember once I was so wrought upon by some trifling thing said or done by one of my relations that I kicked out the bottom of a cane-seat chair I was resting my feet on, in a sudden paroxysm of impotent emotion."

About this time he also made a futile attempt to poison himself by drinking a bottle of strong tincture of valerian that he had made himself. This incident he describes, and then proceeds:

"It was my intention when I began this sketch of my life to give greatest prominence to that part beginning with my troubles in Clinton Street, that is to say, the period of confirmed lunacy with hallucinations, according to the world's avowed decision; but it appears at present that my project is not to go into fulfilment. I have been greatly delayed in doing as much as I have by lack of strength.

"To make the account which I have given as full an exhibition of my condition at the time my hallucinations (if such) appeared, I will note some further defects in my mental action which I had noticed up to this time. First, two or three things indicating original lack of control over the brain by the will, or non-identification of my will with the action of my brain, and which I must count for predisposition. I have been troubled from my boyhood with a tendency of my brain to see things it ought not to see in what is placed before my eyes. This refractoriness does not extend to all kinds of monstrous visions, but is limited to the singling out of the lineaments of the human face in the outlines of objects seen. The annoyance I have experienced from this has varied greatly, according to the state of my health. When I used to be sick with the fever and ague, I would lie in bed and gaze at the coarsely-daubed window shades in my bedroom, until I had made out every possible kind of a profile that could be distinguished.

"The other of the two most serious abnormal peculiarities is the supplying of missing articulations to vocal sounds, heard but not understood distinctly, so as to give my mind the impression of certain words, at the same time that I knew I had not understood. Sometimes I have been really cheated this way, and only found it out by inquiring afterward. This might not give conclusive proof of the deception, it is true. Not to violate privacies, I will illustrate supposititiously. If it were proclaimed aloud, far enough from me to allow the inflections but not the articulations to reach my ear with certainty:

WE SEE WHERE LIES THE DREADFUL SECRET!

My mind might involuntarily and instantaneously reshape it in such a way that I would understand:

DECEIVE WHERE LIES WERE EVER SACRED!

"My attention was always quite easily disturbed by noises, particularly talking. In boyhood the sound of voices in conversation at a little distance after I had retired to rest often gave me very serious annoyance, showing excessive irritability of the brain.

"Such was my mental state on the eve of my being overtaken by a more marvelously awful fate than ever fell to the lot of mortal man.

"My original purpose was to follow the incidents having a bearing on my mental fortunes with tolerable minuteness, in an unbroken chain, up to the time of reaching that wonderful state in which I have existed for the last six and one-half years.

> "I shall be obliged to confine myself more to generalities.
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> "I was in such a towering state of morbid sensitiveness, that a slight tinge of impertinence, brusqueness, or fancied contemptuousness, in the manner of those I met, put me on the rack at once. . . . It began to occur to me after a little that my ears were becoming wonderfully acute for such things. Very often I would hear lively discussions on my character, and disputes about the proper epithets and titles to be applied to me, which I understood perfectly at an astonishing distance off. . . . I was wrought up to such a pitch that I formed a resolve that if I were given a sufficiently open provocation, I would attempt a bloody revenge, and on one occasion went out with a razor in my pocket. . . . I had an oppressive feeling of impotence, as if paralyzed, and suddenly did things I had no intention of doing, as in breaking glass. . . . I had a soreness all through my limbs which I compared to molten fire running through my nerves.
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> "I began to hear responses to and comments on my performances, and it gradually dawned upon me that I had been making myself a conspicuous object of curiosity to the whole neighborhood. . . . The comments heard grew more numerous and more and more derisive. . . . I had no suspicion at the time of any of the inspiration being drawn directly from my head. I do not say it was so. This is the debatable ground. . . . It was not until about a week later that it became evident to me that I was hearing my own thoughts given expression to by foreign wills and voices."

There is a chapter or two of the volume given to illustrations of his hallucinations at this time, but, full of interest as they are, there is no space for their reproduction here. We have followed the author's history thus far from infancy through childhood and youth to manhood, and have seen how slowly but surely the hereditary seed sown in degenerative soil had taken root and flourished. His peculiar auditory acuteness, with his morbid shyness, soon gave rise to illusions of hearing, and these again were transformed into hallucinations, as is evident if the thread of the narrative has been carefully followed. The curious foundation of his hallucinations he well illustrates and understands. An idea rises in his own mind of what people would say in discussing him, and immediately consciousness in the auditory area projects the idea in spoken words into the environment. He noted this peculiarity of his own thoughts being repeated to him by the voices about him, yet he could not correct the delusions to which they gave origin, but interpreted the matter with the reason and judgment of an insane mind. He naturally had the delusion founded upon his hallucinations that people were persecuting him, but upon this now grew another delusion. He began to believe that they could read and repeat his thoughts, that there was some magnetic means by which his tormentors could draw off his thoughts, that other wills could act upon his body, dominating his own will and causing him to do things he had no desire or intention of doing. I will allow him to continue in his own language:

> "I heard a great deal about 'inducting,' 'conducting,' 'sphere of influence,' sometimes even 'poles,' positive and negative, and my brain was constantly compared to a magnet. . . . . I could find no better explanation myself for a long time than the theory of a fluid, similar to or the same as electricity, uniting brains."

Among the extraordinary tales he heard at this time was the following:

> "One was the story of an English physician who had become acquainted with my magnetic properties, and who was on the spot at the beginning, directing the experiment. He was stated to have been the first to form a perfect communication with the inducted

brain, and he had drawn off my entire memory back to childhood, and had delivered it verbally in the presence of reporters from the city, who had taken it down. It was stated that the record was preserved in a number of thick volumes. These he had taken with him when he sailed for England during the most prosperous part of the experiment. It was further asserted that he continued in communication with my thoughts, and that wherever he went, every one to whom he told the story of the new marvel was also set in connection with the magnetic current flowing from my head, and began to participate in my thoughts. . . . . One word more of the English doctor. He is said to have declared that if he had assisted at my birth he would not have suffered me to remain alive, as the monstrous character of my organization could have been seen at a glance. . . . . After the whole earth had become pervaded with the magnetism from my head, it would be felt as long as I lived, and the instant of my death would be thus signaled all over the globe, and would be noted and used by all nations as a new era from which to reckon time."

It was about this time that he was removed to the asylum at Poughkeepsie. Several chapters of his book are devoted to a description of his life there, his religious beliefs, illusions and hallucinations, but from these I can make only a few extracts. A short time previous to his departure for the hospital he began to read much in the Bible, Of this he speaks as follows:

"I would think of the Bible, go and open it at haphazard, and just where my eye fell there was a passage that showed me *myself*. Once when I had been fretting about my ill success in getting my mother to accord with my views about my neighbors' doings, I hit upon this:

'And it shall come to pass that when any shall yet prophesy, then his father and his mother that begat him shall say unto him, Thou shalt not live; for thou speakest lies in the name of the Lord; and his father and his mother that begat him shall thrust him through when he prophesieth,' etc.—Zechariah xiii."

There were other similar coincidences, and he looked upon them at first as merely such, but in time the resemblances became so strongly marked to his disordered intelligence, that he came to look upon whole chapters of the Bible as referring to himself. He says :

> " But the most perfect identity of all is to be found scattered through the Psalms," [of which he quotes several pages and then continues], " I do not intend to appropriate the spirit of these passages, or to make their language my own, but quote them thus collectively as an evidence of fact. I am myself but an enquirer. Do they express the experience of any certain person or persons ? Or are they prophetic ? . . . Can it be that the same thing that has happened to me has befallen another in ages long past, and that these are the traces of it ?
>
> " I have also found a most remarkably close application of many of the precepts and reflections of Thomas à Kempis in his 'Imitation of Christ.' He seems to keep the same character exhibited in the Psalms in view, only speaking as a monitor instead of in his person. I presume I find myself mirrored in both these places, because I am an extreme case."

He also found references to himself in Goldsmith's "Animated Nature," his own surname being there frequently used as an ordinary noun, although a word seldom employed in the English of to-day.

Gradually his delusions burgeoning one from another became so systematized that in the last year of his stay at this asylum he could write in his book :

> " The signs are too many and too evident to permit me to doubt that my destiny is bound up with the religion of the world. I steadfastly believe that the words in Jeremiah, 'take forth the precious from the vile,' are addressed to me; and I cannot be recreant to the holiest of duties. . . . I will not waste time in useless discussion, but start with the assumption that it is God's will that I should give the world my opinions.

"If it comes to be generally believed that my sign is a fulfilment of Hebrew prophecy, I would recommend a transfer [of the Sabbath] to the day of the commandment. The very fact of a day one step removed being fixed on by both Christians and Mohammedans looks like an admission that another step remained to be taken.

"Was it not the confidence of Jesus in the book spoken of above that made him say he knew the Father, when contending with believers in personified derangement?"

Quite a large part of the volume is devoted to expounding the Scriptures, in accordance with his delusion that he is a prophet come to reveal a new religion.

For instance, of Babel he says:

"I find an application for the tower of Babel in my own insane history. I expect a confusion of the speech of the old sects to ensue likewise."

Of Abraham he remarks:

"Abraham is accounted the father of all who believe in the Eternal. I believe I am chosen as his sign for the abolition of all dishonoring beliefs, as Abraham was set up against all idolaters and pagans. . . . I have to note in connection with the offering of Isaac by Abraham, that I find the date given as 1872 before Christ, coinciding with the year after Christ in which my ear-troubles commenced."

Of Esau:

"We may take Esau for polytheistic religion, recognizing and deifying every force and passion that has dominion over the soul or destiny of man. . . . When it gave up its birthright for belief in a single judge, it pledged itself to go on and submit to be judged by the new master. I believe that the day of judgment has come."

Of the miracle of the rods:

"The rods changed into serpents signify arguments becoming living convictions in the mind of Pharaoh.

The evangelists' rods live as serpents in the minds of Christian believers, but I confidently expect that my rod will become a serpent that will swallow them all without trouble.

"Israel is held responsible for the destruction of the heathen and their idols. I conceive that I am the Lord's instrument for the completion of this work, and that I have been shown these signs in the law that my hands might be strengthened.

"I cannot shut my eyes to the fact that I have been made the world's sin-offering."

Of the prophets:

"The prophets I will take in a lump, with the assurance that no one can fail to see their connection with my destiny. There is a prophecy in Ezekiel, xxxiii. 30, which is very closely paralleled in my experience. . . . Jonah gives me a parable."

His discussions of theological questions are interesting, perfectly coherent and logical, although often fanciful. He pays tribute to the beautiful moral laws and righteousness of Christ, but is disposed to criticize his conduct as being inconsistent in one who claimed to partake of the omnipotence and omniscience of the Eternal. Of resurrection he says:

"If I conceive of a new body having the memory which I have of this body's life (and I can find no other idea of the continuance of a soul's life except in the perpetuation or renewal of the memory), would that in the new body be a *true* memory? Would it not be a hallucination? Would not that be an insane creation?"

We see from all that has thus far been quoted that our author had several incentives for writing this book. It contains the autobiography of a new prophet as well as the revelation of a new religion. From his standpoint as a man in whose destiny are wrapped up the destinies of the world, he tells posterity of the tortures and trials he has passed through as an atonement for the sins of the earth, how he was mocked at and scoffed at, his brain acted upon by magnetic agency, and himself imprisoned in a lunatic asylum for years. Hence the title of his book: "*The Piling of Tophet and the Trespass-offering.*" But behind this insane egotism there shines at times some faint glimmer of the truth, so that he frequently speaks of himself, in the terms used by his fellows, as insane, a lunatic, a monomaniac, as having hallucinations; and he thinks the opinions of his friends, relatives and physicians, of sufficient worth to merit considerable argument in his book. He knows what insanity is; he recognizes it in his asylum associates. He could at times "see the man he ought to have become rising up like a shadowy phantom in judgment on the wreck which he really was." But this occasional consciousness of their disordered mental condition is by no means infrequent in the insane. In speaking of the years of his greatest mental aberration, he says:

> "Here I come to more debatable territory, on which I and the rest of the world have until this present been at variance. I will, in deference to the other side, make use of the word *believe* in stating facts drawn from the region of my memory lying within this shadowy world. I will be permitted to say, therefore, that I *believe* that after settling down in the before-mentioned place, my brain was, by the gradual progress of events occurring naturally and according to the ordinary laws of human affairs, drawn into relations to the living actors around me, of an altogether unexampled kind—at all events different from anything plainly recorded in the annals of past ages. I *believe* that the final result of such relations was the superinducing of a state of mental intercommunication through the medium of my sense of hearing.
>
> "But this is a very old story, and merely a re-state-

ment of the perfectly well known features of my alleged monomania. Let me pass on and give as well as I am able my own theory on which I explain these phenomena, which may have more interest. It is a question of personal identification. How does a man use his own brain? He can use it because it recognizes the actions of his members as belonging to the personal unit of which it forms the summit. Now the question is, cannot a human brain under certain circumstances become so perverted as to recognize for itself, and without the volition of its bearer, the acts of other individuals as belonging to its life, as falling within its own memory? And if so, would not those individuals become partakers of the intellectuality of that brain, know its conceptions and ideas, while it thus recognized their motions, and become able to share its walks and ways? Such I believe to have been the result in myself, from the towering height of disintegration reached by my mental organism, by the gradual process which I have endeavored to faintly shadow forth in the preceding five chapters.

"Let us see whether it does not look probable that a mind in the habit of separating recognized observations from its own responsibility, considering them objectively, philosophizing on its own manner of working, driving the impotent and erratically-acting part into a corner, as it were, would not be more exposed to such a fate as supposed than one acting unitedly, and right or wrong as a unit. It may not be susceptible of argument based on points of organic action, but it looks a plausible thing to me that the insane quality or element in such a brain might be acted on from without, and give itself up to such action, independent of the thinking will of that mind.

"But let us further suppose some little abnormality about the original constitution, a predisposition from a slightly dislocated arrangement of mind-apparatus and sense-apparatus.

"Such, say I once more, I believe to have been the case with myself, and such to be the true nature and essence of the things which have constituted my insanity. . . . . I do not deny the fact of insanity, but I firmly believe that it is and has been, since the summer of 1872, an insanity involving the will, ideas and acts of more than one individual.

"Notwithstanding my full and necessary faith in the reality of things as I have reasoned to prove them, I am still willing to concede that there has been more or less of purely subjective illusion mingled with these dual realities. Under one aspect the whole of this train of mental images and impressions which has whirled through my head has consisted of insane delusion. The effect on the state of my system has no doubt been analogous to that produced by delusions, and the nervous condition which preceded it was such as eventuates in the rise of delusions. Does not the development of delusions often have a compensating effect in freeing the nervous system in a manner from its trammels? Perhaps when this supervenes, the brain becomes a chimney for the combustion of the matters which threatened to entirely interrupt the action of the system by clogging. The patient is then known as sensible on most subjects, but a confirmed monomaniac."

Certain peculiarities in his hallucinations possess considerable interest. They almost always referred to the intercommunication of brains. In July, 1878, he wrote out a list of specimen phrases which he had heard while sitting alone at an asylum window. Some of these I reproduce here:

"One thing you know, you know when you get your will in there you get him into a hell of misery."—"He ain't got any will there to fool away."—"Although you are knowing his ideas you connect with her will."—"Instead of connecting with his ideas you keep giving him to her."—"You can't get your will there till he connects his thought to his thought."—"We are all the while trying to make him think himself."—"I think we ought to be making efforts to get the idea out on the hall."—"After they get the whole will he is in a hell of torture all the while."—"We keep hollering till we get him into a hell of horrors."—"You see, when there are two wills connected with the head at the same time, he ain't nowhere."

These were the voices of several men and women. In fact, his hallucinations were always polyphonic, and at times would be polyglot. They did not address him directly, but spoke to each other about him. He seldom had hallucinations of hearing, except when the ear actually received the sound of distant conversation or inarticulate noises; so that for their production it was usually necessary that there should be transmission of vibrations to the auditory cortical area. As instances of the polyglot character of the voices on occasion, I relate the following:

Once he heard some one call out, "If he ain't a prophet there never was a prophet—*tabulas dedi ut vincerer.*" In tracing this Latin to its source, he found it was a perversion of a phrase in a note to Whiston's Josephus: "*Egomet tabulas detuli ut vincerer*" (I myself carried the letter commanding that I be bound), attributed to Bellerophon, which he had once read.

At another time in a street car, a German sitting next to him cried out, "*Das ist das grösste Mirakel von der ganzen Welt. Jeder Gedanke der ihm in den Kopf gekommen ist hat die ganze Village gehört.*" (That is the greatest miracle in the world. The whole village has heard every thought that has come into his head.) The grammatical construction of the foreign phrases is open to criticism. The language used by his invisible tormentors was always a peculiar dialect, often abounding in slang, which he considered the most hateful kind of language, and which was such as he never voluntarily used in the composition of his own sentences. The hallucinations were usually boisterously satirical, teasing, quizzical, frequently accompanied with laughter.

It is seen that the author of "The Piling of Tophet" was one of the most typical cases of a chronic primary insanity, as described by Morel, Sander, Krafft-Ebing, Mendel and others. Born with a constitution in which lurked a degenerative taint, he passed his youth as a hypochondriac, the victim of a morbid subjectivity, and on the eve of ripened manhood developed auditory illusions, which became slowly hallucinations, with the simultaneous evolution of delusions of persecution, followed later by systematized expansive delusions of a religious nature.

Shortly after writing his autobiography, he was removed to the county asylum at Mineola, where he remained without change in his mental condition until 1883, when his friends took him out to live with them. He spent eleven years in asylum life. He died a religious paranoiac in 1886. He did not become demented, but doubtless the indifference with which the world received the propagandism of the new prophet caused his philosophical withdrawal from active warfare in the fields of reform and theology.

MEMORY, HISTORICALLY AND EXPERIMENTALLY CONSIDERED.

W. H. BURNHAM, PH. D.

II.

MODERN CONCEPTIONS OF MEMORY.

I.—*Disciples of Hartley.*

The writer last noticed in this sketch of the conceptions of memory was David Hartley. Before passing to the Kantians and the Scottish School, an Italian philosopher should be mentioned, who, whether a disciple of Hartley or not, taught an associational psychology very similar to that of the great English psychologist. I refer to Francesco Maria Zanotti, whose little treatise, *Della forza attrativa delle Idee*, appeared under the false date of 1747, purporting to be the work of a Frenchman.[1] This work contains Zanotti's views of memory.

Zanotti agreed with Malebranche in considering memory as a kind of habit. In his opinion, it is not a faculty. A faculty is given us by nature; habit is acquired. Memory comes in this latter way. It is formed little by little, by the repetition of feelings and thoughts. While explaining memory by the association of ideas, he invented a theory, not found in Hartley, to explain association. The attraction of ideas is the cause of association. What was little more than a figure of speech with Hume, became a psychological principle with Zanotti. He speaks not only of the attraction but of the electricity and magnetism of ideas, and he formulates the law that *the attractive force of ideas will be proportional to their fullness or perfection.* This attraction of ideas explains memory. According to Zanotti, "when we unite the idea of a certain thing with that of a certain time, these two ideas acquire by contact a kind of magnetism which causes one to attract the other; thus the force of reciprocal attraction is produced in the needle and the magnet when they have once been placed in contact. In the same manner, when the idea of a thing is awakened in us, it draws after it that of the time with which it has once been joined; and it is in this attraction that memory consists. Thus the thing reminds us of the time; and not less frequently the time reminds us of the thing. This is equally true of place; for the memory of a place brings back to us that of the event which happened there, and that of the time in which it occurred. These ideas of thing, of time, and of place having been once united, have acquired a sort of friendship—have become, so to speak, magnetic, and have begun reciprocally to attract one another."[1]

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[1] See Opere di F. M. Zanotti, tomo V, Bologna, 1790; also Ferri: La psych. de l'association, ch. III

Among other disciples of Hartley may be mentioned Priestley and Erasmus Darwin. The last mentioned distinguishes ideas that we voluntarily recall as "ideas of recollection," those that recur involuntarily as "ideas of suggestion."[2] He argues vigorously for the theory that recollection is reproduced movement. He says: "If you wonder what organs of sense can be excited into motion when you call up the ideas of wisdom or benevolence, which Mr. Locke has termed abstracted ideas, I ask you by what organs of sense you first became acquainted with these ideas? . . . . If our recollection or imagination be not a repetition of animal movements, I ask, in my turn, What is it? You tell me it consists of images or pictures of things. Where is this extensive canvas hung up? or where are the numerous receptacles in which those are deposited? or to what else in the animal system have they similitude?"[1]

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[1] Ferri, op. cit., p. 58.

[2] Zoonomia, Vol. I, §II, 2, 10.

Abraham Tucker, another disciple of Hartley, deserves mention; for, while holding that an idea is a reproduced movement or cerebral modification of some kind, he avoids some of the objections usually brought against a physical theory of memory. He does not think that the motion or modification caused by our seeing an object persists until we see the same object again, perhaps a year afterward. Such a view would render necessary the supposition that "our internal organs must be as numerous as the ideas we possess." "But," he argues, "one substance may be susceptible of various modifications at different times, and as the same optic nerves serve to convey red, yellow, or green, according to the rays striking upon them, so the same internal organs may exhibit various ideas according to the impulse they receive from elsewhere."[2] That is, as I understand the passage, the same nerves have the power of vibrating in different ways as the external stimuli vary. In like manner, the same nervous substance of the brain has the power to reproduce different vibrations as the stimuli vary; the stimuli in the latter case being, of course, either the sensations from external objects or associated ideas.

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[1] Op. cit., §III, 6 & 7.

[2] Human Nature, Ch. VIII, §3. But see also §2, where he finds the analogue of memory in the visual after-image, and says that "our mental organs have a like quality with the bodily, of conveying perception to the mind when the causes setting them at work no

II.—*Kant and His Disciples.*

Kant's views of memory are presented at some length in his later writings.[1] But, indirectly, much is said about memory in the Critique of Pure Reason, in his discussion of the relation of the imagination to knowledge.[2] It is not necessary to present Kant's theory of knowledge here, but it will be remembered that the imagination mediates between sense and the understanding, partaking of the nature of both. It is allied to sense because we can imagine only sense-perceptions; it shares the nature of the understanding because it synthesizes the manifold of intuition according to the categories, and this synthesis is an exercise of the spontaneity of the mind. Kant distinguishes two forms of the imagination—the *productive* and the *reproductive*. So far as the imagination is an exercise of the spontaneity of the mind, clothing the pure concepts of the mind in sensuous form, he calls it *productive*. So far as it merely revives past sense impressions it is *reproductive*. The former acts according to universal and necessary laws; the latter, according to the empirical laws of association. The former may go

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longer operate. For what the impulse of objects is to the optic or auditory nerves, that the impulse of these latter is to the mental organs; yet we see the idea of an object may be retained after both those impulses are over."

[1] See Anthropologie, §§27-33; also, Pädagogik, A.

[2] See the Transcendental Logic, passim.

beyond experience in the presentation of pure intuitions of space and time; the latter implies previous empirical perception. The former is the imagination of the poet and the painter; the latter, the ordinary power of any Gradgrind. The relation of the imagination to what is often included under memory is evident from Kant's definition of the former. "*Imagination*," he says, "is the faculty of perceiving an object without its presence."[1] He distinguishes memory from the reproductive imagination by limiting the former to the *voluntary* reproduction of a presentation. The productive imagination, he says, if mingled with memory, renders it false.[2] The highest act of memory, that of recognition, is, in Kant's psychology, a transcendental act of the ego.

Kant distinguishes three modes of memorizing—the mechanical (*mechanisch*) method, the method of clever devices (*ingeniös*), and the method of reflection (*judiciös*). "The first consists merely in frequent, literal repetition." The second is a method of learning given ideas by associating them with others, however incongruous, thus burdening the memory by the arbitrary association of disparate ideas. The third method is the best. It consists in classification, and will enable one by reflection to recall what is wanted.[3] Kant has no especial praise for the mnemonic art, and memorizing for mere discipline he deems useless. But in the Pädagogik he gives some advice for cultivating a child's memory.[4]

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[1] Kritik der reinen Vernunft, Supplement XIV: "Einbildungskraft ist das Vermögen, einen Gegenstand auch ohne dessen Gegenwart in der Anschauung vorzustellen."

[2] Anthropologie, §33.

[3] Loc. cit.

[4] Pädagogik, Sämmt. Werke, Vol. IX, p. 403 (Rosenkranz-Shubert ed.).

Fichte develops the theory of memory involved in Kant's doctrine of the imagination.[1] According to his idealistic psychology, the power of imagination arises through the development of freedom in reflection. The imagination has the free power to renew external perceptions. In fact, we might say that the imagination is this power of renewing perceptions, for further activity of the imagination would be merely idle play. The possibility of reproduction of perceptions by reflection is always present. But this possibility is merely a norm of thought, according to which a perception would be actually reproduced. The gist of this norm is to direct the imagination how to limit itself self-actively in its survey of the whole field of reproducible perceptions. Such reproduction implies classification and keenness of sense-perception. The external senses do not give us the renewed perception, hence the imagination must have the power by its own causality of renewing the different elements of the original perception. The immediate perception differs from the reproduced picture in this: the latter is always accompanied by the consciousness of self-activity. The ego feels that it makes every feature of the picture. The actual perception, on the other hand, is always accompanied by the consciousness of lack of freedom. The act of reproducing a perception is, to quote Fichte's words, "a self-limitation of the imagination within its whole field in accordance with a limitation of the outer sense. The norm (*Regel*) of this limitation is the concept, namely, of the object of the external perception which is reproduced."[1] Fichte distinguishes two cases of the reproductive imagination, namely, when the reproduced impression is joined with the concept, and when it is not. In the latter case we have *unconscious reproduction;* in the former, *remembrance.*[2]

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[1] See Die Thatsachen des Bewusstseins (Vorlesungen, 1810–11), Ch. IV, and Zweiter Abschnitt, Anhang über das Erinnerungsvermögen. Also Die Thatsachen des Bewusstseins (Vorlesungen, 1812–13), Vortrag IX.

The aim of reproduction of perceptions is to get the external world independent of itself in our power. Henceforth we are free in reference to the world. We may let its stream flow or stop it at will. Hence the importance of cultivating the memory.

Fichte was always at his best when he left the transcendental heights of his philosophy and talked about practical matters. In his chapter upon the reproduction of impressions, he does not fail to add some good practical suggestions. It is well to have the parts of our free self-built world in a firm and lasting form. The imagination left to itself easily becomes confused. It must be limited. *Writing* is the means by which we may bind our wandering thoughts. If our thoughts are vague, we shall notice it when we write them down or when we test what is written.[3] For making the reproduction of visual impressions definite, Fichte also recommends *drawing.* Practice is the great means of strengthening the power of recollection. Fichte even says that "special natural endowment, talent, genius, or whatever we may call it, has no influence" on this power. The power of recollection is not an accidental phenomenon; "but it is a necessary and inseparable part of consciousness." Hence anything that benefits the mind is good for the memory. "The clearer, freer, and more self-controlling consciousness is in general, the more comprehensive and ready is the power of recollection. (The true principle of a system of mnemonics is the maxim, *sapere aude*)."[1]

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[1] Thats. d. Bewusst. (Vorles. 1810–11), p. 43.

[2] Thats. d. Bewusst. (Vorles. 1812–13), Nachgelassene Werke, p. 494.

[3] His aphorism is: "Ein gründliches und durchgeführtes Denken wird, meines Erachtens, kaum anders gelingen, als mit der Feder in der Hand." Loc. cit., §9.

Schelling has nothing to say upon the subject of memory that need detain us. F. A. Carus,[2] however, a disciple of Kant whose writings are little known in this country, treats the subject of memory more fully and, in some respects, more satisfactorily than either Kant or Fichte. He holds an idealistic doctrine analogous to the doctrine of physiological psychology, that all our experience modifies the brain-centers in their growth. Memory means continued assimilation. Everywhere in nature, action implies reaction. In the subjective world this reaction, according to his view, is an effect persisting in time. "The continued life of man is, however, an advance from an original state of indefiniteness and dependence to a condition of greater and greater definiteness and stability. . . . Hence the ability to appropriate experiences, changes, developments, and what is received from sensation or feelings, and to cause it to persist as one's inalienable inner possession. Even this ability may be looked upon as the condition of memory, if not as memory itself. What has been experienced continues to live in us. But nothing that persists in us enters the sphere of our consciousness, consequently not into the higher sphere of activity of the mind and will, nor into our free power, without continued activity; thus what has been experienced does not enter consciousness without reproduction, without being made our own by repetition." Memory is not a mere power of receiving and of preserving impressions; "it is rather a power of making present and living what has been perceived, thought, felt, and willed, together with more or less of a revival of space or of time." Finally, memory is not a separate faculty, but is involved in the other powers of the mind.

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[1] Anhang über das Erinnerungsvermögen.

[2] See his Psychologie, Bd. I, p. 217 seq.

Hegel's doctrine of memory is so interwoven with his metaphysical system that space cannot be taken for a presentation of it here.[1] But what has been said of Fichte's views is sufficient to show the general character of the German idealistic psychology of memory. The most important feature of it is the emphasis placed upon the *spontaneity* of the mind in memory. One other important part of this doctrine, however, must not be omitted.

How the mind can preserve ideas has been the problem that philosophers have wrestled with or ignored ever since the days of Plato. Some of the German idealists attacked the problem and attempted to explain this mystery of memory; or rather, like Diogenes, they reversed the problem and attempted to explain forgetting. Fries, professor at Heidelberg before Hegel, is a good exponent of their views.[2] He maintains, with Locke, that there is an internal sense that takes cognizance of our mental activity. He adopts the distinction usually made by careful writers, between memory and the power of recollection. Memory, according to his definition, is merely the power of preserving impressions that we have once had. Provided we do not mix the physical and the mental, it is his opinion that pure introspection will show that there is no difficulty in the theory of memory. Every cause in nature, once set in action, continues until checked by other causes. This law of inertia holds in the mental world as in the physical. Hence, "not memory and the preservation of presentations that have once been had, but the forgetting of them, requires special explanation."

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[1] For a discussion of the subject from the Hegelian standpoint see Rosenkranz's Psychology, pp. 267–327.

[2] See his Neue Kritik der Vernunft, Bd. I, §§29–36; Psych. Anthropologie, Bd. I, §§10, 11, 43; Logik, §§10, 132. Cf. the views of Carus given above.

To solve the problem of forgetting, we must notice the distinction between our clear and obscure ideas, or the relation of the inner sense to memory. Only the most lively activities affect the inner sense sufficiently to appear in consciousness; the quietly continuing changes remain for the most part obscure. "Hence we always notice our memory only in its influence on the internal perception of presentations. All ideas that we once have or have had, remain present to us; the whole stock of our presentations remains in the memory. But, as new impressions are continually pressing in upon the old and sharing the presenting power of the ego, this becomes more and more divided, the previous presentations are thrust back and very soon obscured. Hence, if earlier presentations are not from time to time renewed they gradually become weaker and weaker. In this manner we must conceive that our whole knowledge is always present in the mind, but that at each moment only very few presentations have sufficient clearness to reach consciousness, *i. e.*, to be perceived. Hence we can regard forgetting as only a gradual obscuring of our presentations, without needing to suppose that they are ever entirely lost to us."[1]

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[1] Neue Kritik der Vernunft, Bd. 1, §30.

Recollection, or the return of obscured presentations to consciousness, according to Fries, is not properly a reproduction, but a reappearing. The two causes of recollection are an increased sensibility of the inner sense and an increased vividness of ideas brought about by association. In the phenomenon of association we find one idea so bound with another, that if one becomes clear again, the other grows clear also. "The law of the strengthening of one presentation by others, with which we here have to do, is the opposite of the law of the weakening of presentations that we found in the study of memory. Both rest on the meeting of presentations in one life-state, and in their affinity in respect to their origin. If one presentation meets another, then it weakens it, since both must share the mind's power in an equal degree. But if they have previously been together, and one is again strengthened, then it imparts this strength also to the other. . . . The different activities which the mind manifests at the same time belong to one act of the mind; the strengthening of a part of this activity is at the same time a proportional strengthening of this whole activity; that is, inner activities affect each other more or less by the law stated, according as they belong together in one action of the mind."[1]

This doctrine of memory was afterwards taught by H. Schmid,[2] and was adopted by Sir William Hamilton.[3]

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[1] Op. cit., §33.

[2] Versuch einer Metaphysik der inneren Natur, pp. 231-235. Quoted by Hamilton, Lectures on Metaphysics, L. XXX.

[3] Works, loc. cit.

III.—*The Scottish School.*

The views of Hume have already been noticed. Among other early Scotch writers who have something to say of memory or of the association of ideas are Hutcheson, Turnbull, and Henry Home. But the first representative writer of the Scotch School is Reid. He discusses memory at some length, but does not throw much light upon the subject.[1] "It is by memory," he says, "that we have an immediate knowledge of things past." In his opinion, "memory is an original faculty, given us by the Author of our being, of which we can give no account but that we are so made." The distinguishing feature of memory is that it implies belief in what we remember. Yet, sometimes "the memory of a thing may be so very weak that we may be in doubt whether we only dreamed or imagined it"; and children may be subject to illusions of memory. "Our notion of duration, as well as our belief in it, is got by the faculty of memory."

In discussing the subject of memory, Reid made some statements that are astonishing to most philosophers. "The knowledge which I have of things past, by my memory," he says, "seems to me as unaccountable as an immediate knowledge would be of things to come." He thinks it remarkable that men have found difficulty only in reconciling prescience with free acts. He maintains "that we can as little account for memory of the past actions of a free agent. If any man thinks he can prove that the actions of a free agent cannot be foreknown, he will find the same arguments of equal force to prove that the past actions of a free agent cannot be remembered."[1]

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[1] See Essays on the Intellectual Powers of Man, Essay III.

Reid maintains that association and vividness of ideas cannot account for memory. He appeals to consciousness to decide the point. "I could as easily believe," he says, "that a hat is a pair of shoes as that memory is a certain degree of vividness in ideas and of strength in their association." "A malefactor that is going to be hanged has a cluster of very vivid ideas, and very strongly associated, of what he is about to suffer, but it is not the object of remembrance but of foresight."[2]

James Beattie, a contemporary of Reid, wrote a popular dissertation upon memory, in which he repeated the usual stories and platitudes, and gave some excellent advice in regard to training the attention and the memory.[3]

None of the Scottish philosophers have described the facts of memory better than Dugald Stewart. He presents a good analysis of the subject as studied introspectively, describes the different varieties of memory, gives a collection of cases of remarkable memory, and discusses at length the means of improving it.[4] While he confines himself chiefly to the discussion of the well-known phenomena of memory and to the presentation of facts, he modestly presents a theory of the relation of memory to conception that contains the main features of the doctrine of memory developed more recently by Taine.[5]

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[1] Loc. cit., pp. 341 and 342, Hamilton's edition.

[2] MSS. papers by Dr. Reid, cited by Dr. McCosh. See Appendix, Art. III, pp. 473 and 474 of McCosh's Scottish Philosophy.

[3] Dissertations Moral and Critical, Dis. I.

[4] See Elements of the Philosophy of the Human Mind, Vol. I, Ch. V and VI.

[5] Intelligence, Part II, Book I, Ch. 2 and passim.

Conception, in Stewart's psychology, is "that power of the mind which enables us to form a notion of an absent object of perception, or of a sensation which it has formerly felt." In opposition to the generally received doctrine of his day, Stewart maintains "that the exercise both of conception and imagination is always accompanied with a belief that their objects exist." In most cases this belief is only momentary, and is "immediately corrected by the surrounding objects of perception."[1] When we recollect a past event in which an object of sense was concerned, the act of recollection involves an act of conception. But the former implies a belief in the past existence of the object; the latter, a belief in its existence at the present moment. How shall we reconcile this apparent contradiction? "The only way that occurs to me of removing this difficulty," says Stewart, "is by supposing that the remembrance of a past event is *not a simple act of the mind*, but that the mind *first* forms a conception of the event, and *then* judges from circumstances of the period of time to which it is to be referred. . . . So long as we are occupied with the conception of any particular object connected with the event, we believe the present existence of the object; but this belief, which in most cases is only momentary, is instantly corrected by habits of judging acquired by experience."[2] This reference of recollected events to the proper points of time in the past is "strikingly analogous to the estimates of distance we learn to form by the eye."[3]

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[1] Op. cit., Ch. III.

[2] Op. cit., Ch. VI.

[3] According to this view, there are three operations in any complete act of memory: (1) the reproduction of a previous state of consciousness—*i. e.*, an illusion; (2) the rectification of this illusion by the present state of consciousness; (3) localization in the past. See Taine, op. cit.

Thomas Brown, Dugald Stewart's successor at the University of Edinburgh, writes with his usual eloquence of memory. The main point that he brings out is that "memory is not a distinct intellectual faculty," but that "the state of mind in memory is a complex one," resulting from the combination of two elements, namely, a conception and a felt relation of priority. Moreover, he analyzes what he terms voluntary recollection, and shows that it, "whether direct or indirect, is nothing more than the coexistence of some vague and indistinct desire with our simple trains of suggestion."[1] He discusses the time-honored question of the relation of a good memory to intellectual power, and maintains that "it is not a good memory in its best sense, as a rich and retentive store of conceptions, that is unfriendly to intellectual excellence, poetic or philosophic, but a memory of which the prominent tendency is to suggest objects or images which existed before, in the very order in which, as objects or images, they existed before, according to the merely imitative relations of contiguity." Thus the difference between the ordinary man and the genius is, that in the mind of the former, ideas follow each other mainly according to relations of former contiguity, which are limited, while in the mind of genius they are suggested by the relations of analogy, which are infinite.[2]

Sir William Hamilton, the Hercules of the Scottish school, discusses memory and the association of ideas with his usual profoundness and display of erudition.[3]

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[1] Lectures on the Phil. of the Human Mind, Lect. XLI.

[2] Loc. cit.

[3] Lectures on Metaphysics, Lect. XXX, and Notes D** and D***, Ed. of Reid, Vol. II.

Yet his discussion presents little that is new. In the first place, by keen analysis and clear definitions, he avoids the confusion that is liable to arise in the use of the word memory.[1] He uses the word (as the most careful philosophers have done before his time and since) to denote merely retention. He has no good opinion of physiological hypotheses for explaining memory. "All of them are too contemptible even for serious criticism," he exclaims with dogmatic impatience. Nor are they necessary. The phenomena of memory present no difficulty. Forgetting is the puzzle. Here he quotes with approval from H. Schmid the theory of memory explained above in outlining the views of Fries. This theory of the persistence and obscuration of ideas, and his famous law of association by *reintegration*, are the most important features of his doctrine of memory and recollection. It is not necessary to repeat the former here; it is apart from the plan of this article to discuss the latter.

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[1] In the following passage he points out the cause of the confusion in the use of this word: "Because the faculty of Conservation would be fruitless without the ulterior faculties of Reproduction and Representation, we are not to confound these faculties, or to view the act of mind which is their joint result, as a simple and elementary phenomenon. Though mutually dependent on each other, the faculties of Conservation, Reproduction, and Representation are governed by different laws, and in different individuals are found greatly varying in their comparative vigor. The intimate connection of these three faculties, or elementary activities, is the cause, however, why they have not been distinguished in the analysis of philosophers, and why their distinction is not precisely marked in ordinary language. In ordinary language we have indeed words which, without excluding the other faculties, denote one of these more emphatically. Thus in the term *Memory*, the Conservative Faculty—the phenomenon of Retention—is the central notion, with which, however, those of Reproduction and Representation are associated. In the term *Recollection*, again, the phenomenon of Reproduction is the principal notion, accompanied, however, by those of Retention and Representation as its subordinates." Metaphys., Lect. XXX.

IV.—*The English Associationists.*

The English associational psychology, as already indicated, began with Hume and Hartley, while others, notably Hobbes and Locke, had discussed the phenomena of association long before the appearance of the "Observations on Man." The Scottish philosophers also gave attention to the subject, and Thomas Brown might be classed with the associationists, so much importance does he give to what he calls the mind's capacities of Simple and Relative Suggestion.

The real successor of Hartley, however, was James Mill, and the latter treats memory at length.[1]

Mill analyzes the phenomena of memory carefully but not quite thoroughly. He distinguishes two cases —the remembrance of sensations and the remembrance of ideas. In the remembrance of a visual sensation, for example, there is the idea of a thing and that idea brought into the mind by association. "But in memory there is not only the idea of the thing remembered; there is also the idea of my having seen it. Now these two—1, the idea of the thing, 2, the idea of my having seen it—combined, make up, it will not be doubted, the whole of that state of consciousness which we call memory."[2] The last part of this compound is a very complex idea containing two elements—"*the idea of my present self*, the remembering self; and *the idea of my past self*, the remembered or witnessing self." In the moment of memory, "the mind runs back from that moment to the moment of perception. That is to say, it runs over the intervening states of consciousness called up by association.

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[1] Cf. Analysis of the Phenomena of the Human Mind, Vol. I, Ch. X.

[2] Loc. cit., p. 329, J. S. Mill's ed.

But 'to run over a number of states of consciousness called up by association,' is but another mode of saying that 'we associate them'; and in this case we associate them so rapidly and closely that they run, as it were, into a single point of consciousness, to which the name MEMORY is assigned."

Thus the recognition of a *sensation* consists of "three principal ingredients: 1, the point of consciousness called the remembering self; 2, the point of consciousness called the percipient self; 3, the successive states of consciousness which filled up the interval between these two points." In remembering an *idea* the ingredients are the same, except that the second is the "conceptive self."

The phenomena of forgetfulness confirm this account of memory. "Every case of forgetfulness is a case of weakened or extinct association. I cannot remember the discourse that I learned years ago, because the few words that I recall fail to suggest the following words."

Illusions of memory occur when "a case of the memory of *ideas* comes to be mistaken for a case of the memory of *sensations*." This occurs to the liar, when, in continually repeating his story, he dwells lightly upon the idea of himself as fabricating it and strongly upon the idea of himself as an actor in it, until, finally, the strongly associated idea of the latter circumstance overpowers and destroys the weakly associated idea of the former.

To sum up our account in Mill's own words, "Remembering is associating." And memory "is an idea formed by association of the particulars of a certain train—a train of antecedents and consequents, of which the present feeling is one extremity."[1]

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[1] Op. cit., Vol. II, Ch. XIV, §5.

The way in which Bain and Spencer have developed the associationist psychology and filled the gaps in the earlier form of it is too well known to be recounted here.[1] It is necessary to recall only a few important features of their doctrine of memory on its physiological side.

We have already had occasion to note the anticipations, notably by Bonnet and Erasmus Darwin, of Bain's famous doctrine, that when an impression is reproduced, "the renewed feeling occupies the very same parts and in the same manner as the original feeling, and no other parts and in no other assignable manner."[2] This, argues Bain, is the only view compatible with our knowledge of the nerves. In the case of the after-impression of sense, we must infer that the continuing sensation is the result of persistence of the nerve-currents aroused by the original stimulus. If impressions surviving their originals are due to persisting nerve-currents, it is likely that revived impressions are likewise due to re-induced currents, feebler but in the same nerve-tracks as were occupied by the original sensation. Observation confirms this doctrine. The recollection of language is suppressed articulation. The vivid thought of an action impels us to perform it. Lively imagination of a color fatigues the nerves of sight. The thought of laughter keeps the hysteric laughing; and the hypnotized patient acts out all ideas suggested to him.

The physical mechanism of retention is as follows, according to Bain: "For every act of memory, every exercise of bodily aptitude, every habit, recollection, train of ideas, there is a specific grouping or co-ordination of sensations and movements, by virtue of specific growths in the cell-junctions."[1]

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[1] For a valuable presentation of their doctrines, see Ferri: La Psych. de l'Association. Also for valuable additions and illustrations, see Hodgson's "Theory of Practice," Vol. I, B. I, Ch. III, and an article on "The Association of Ideas," by Professor James, in Pop. Sci. Monthly, March, 1880.

[2] The Senses and the Intellect, p. 338.

As retention thus depends on a physical process, it follows that acquisition is limited by the size of the brain. "We are all blockheads in something." Great capacity in one branch of education is apt to be purchased at the price of corresponding deficiency in something else. Yet, from a rough estimate, Bain compares the number of our acquisitions, motor as well as sensory, with the number of cells and fibers in the brain, and concludes "that there is no improbability in supposing an independent nervous track for each separate acquisition."[2]

Herbert Spencer studies the genesis of memory.[3] In the growing complexity of the adjustment of inner relations to outer relations that, according to him, constitutes the evolution of life, there comes a stage when there are only "fragments of correspondences" between the two. At this stage memory appears. In instinct, the process of adjusting internal relations to external relations is complete; in memory, there are at most only partial correspondences. Yet the germs of memory are found in instinct, *i. e.* in compound reflex action. For the nerve center receiving and co-ordinating a series of impressions cannot record all these impressions at absolutely the same instant; yet, as the appropriate reaction is a response to the whole group, it follows that the first impression of the series must persist until the last is received. This persistence is weak memory.

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[1] Mind and Body, p. 91, 2d ed.

[2] For details of his estimate, and for the way in which he supposes the nerve groupings arise and can be isolated, see Mind and Body, p. 96, seq. See also his article on "The Retentive Power of the Mind," Fortnightly Review, Sept., 1868.

[3] Psych., Vol. I, Part IV, Ch. VI.

Furthermore, when psychic states have been infrequently connected in a series, their cohesion is feeble and the transition from each state to the next in the series is slow—*e. g.* in learning a language. This deliberate succession of psychic states is one of the conditions of memory.

Again, it often happens that two groups of impressions, differing by only one or two elements, are responded to by very different reactions; (*e. g.* two animals that look alike, one of which retaliates when attacked and the other does not. Here the groups of impressions from the two animals are, for the most part, alike; but the appropriate reactions are very different, *i. e.* running away in the one case, attack in the other). When these particular groups occur infrequently, the appropriate reaction becomes undecided. For the common elements of the two groups tend to excite either of the two sets of action that have constituted the responses to them. Hence arises a conflict between the psychic states involved in the two movements, and they become merely nascent movements or tendencies. This is the beginning of memory. To quote Spencer's words: "In the chief nervous center the different impressions serve as different motor impulses; and these, being severally supplanted by one another before they pass into the actual motor changes, will each of them consist of an incipient or faint form of that nervous state which would have accompanied the actual motor change had it occurred. But such a succession of states constitutes *remembrance* of the motor changes which become incipient—

constitutes a *memory*. To remember a motion just made with the arm is to have a feeble repetition of those internal states which accompany the motion—is to have an incipient excitement of those nerves which were strongly excited during the motion. Thus, then, these nascent nervous excitements that conflict with one another are really so many ideas of the motor changes which, if stronger, they would cause; or rather they are the objective sides of those changes which are ideas on their subjective sides. Consequently, memory necessarily comes into existence whenever automatic action is imperfect."

Moreover, the sensory memory is a concomitant of the motor memory just described. As the external groups of relations become more complex and infrequent, the corresponding groups of impressions become less coherent, and a nascent memory of the component parts of a group becomes possible. General sensory memory is a derivative from this; "for the same progress which gives the ability to receive the complex impressions required to determine complex actions, gives the ability to receive complex impressions which do not tend to determine any actions at all." The latter class of impressions having no direct connection with action, have direct connection with each other in varying degrees of constancy, and tend to arouse one another in varying degrees. Hence arises that succession of ideas we call memory.

While "memory comes into existence when the involved connections among psychical states render their successions imperfectly automatic," the obverse of this is true; and when by multiplied experience the appropriate response to outer relations becomes structurally registered, then conscious memory passes into unconscious or organic memory. Thus, while "instinct may be regarded as a kind of organized memory, on the other hand, memory may be regarded as a kind of incipient instinct."

V.—*Herbart.*

Turning back to Germany, Herbart and Beneke are the first prominent representatives of the "new psychology."

In the psychology of Fries we have already seen foreshadowed that theory of the inter-relation and ceaseless rise and fall of presentations over the threshold of consciousness that constitutes the static and dynamic of Herbart's psychology. But Fries emphasized the spontaneity of the mind, Herbart its receptivity. According to Fries, a presentation returns to consciousness because strengthened by another; according to Herbart, because an antagonistic presentation is arrested.

According to Herbart,[1] what occurs in the constant meeting in consciousness of new presentations with the old is this: similar presentations blend; contrary presentations mutually arrest each other; disparate presentations are complicated. But even arrest does not involve annihilation. The weaker presentations are obscured, but they persist as tendencies, and as soon as hindrances are removed they become actual; for presentations, by their very nature, are always striving for self-preservation. Reproduction is either immediate or mediate. It is immediate when presentations reappear in consciousness merely by reason of the removal of hindrances, and without help from presentations that have previously been blended or complicated with them. It is mediate when a presentation reappears in consciousness by the help of other presentations previously combined with it. A series of presentations is reproduced in consciousness in the same order in which it was originally perceived, for this reason : each successive member of a series blends with all the other members of the series that are above the threshold of consciousness, with an energy proportional to the vividness of the latter. For example, in series α, β, γ, δ, suppose that the presentive activity of β is at a maximum and α is sinking, when γ appears : then γ will combine closely with β, lightly with α. When δ appears, the presentive activity of γ is at a maximum, β is sinking, and α is fading from consciousness; consequently δ combines closely with γ, lightly with β, very lightly with α; conversely α, which combined closely with β, combined lightly with γ, and very lightly with δ. Hence, after the series has vanished for a time from consciousness, and the initial member is in some way reproduced, α recalls β with an energy greater than that with which it recalls γ, and so on; thus the series is reproduced in its original order.[1]

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[1] Cf. Lehrbuch zur Psych., passim; also, Psych. als Wissenschaft, passim. For a good account, in English, of Herbart's psychology, see also articles by G. F. Stout, in Mind, Nos. 51 and 52.

Thus in Herbart's psychology memory is no innate faculty, but is involved in the striving of presentations for self-preservation. Forgetting is the arrest of weak presentations by stronger presentations, and occurs because the simplicity of the soul makes it impossible for contrary presentations to coexist in consciousness. The obscured presentation has a latent power analogous to latent natural forces. Reproduction is the natural result of the irrepressible striving of presentations, and occurs *ipso facto* when obstacles are removed.

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[1] Cf. Stout on the evolution and involution of series, op. cit., No. 51, pp. 334, 335.

Beneke agrees with Herbart in combating the idea that memory is a faculty; and, with some change of terminology, his theory of memory is, in general, not essentially different from that just given, except that, according to him, presentations are not really in conflict, but a presentation becomes obscured because certain psychic elements are transferred from it to others, and it returns to consciousness because it receives an increased quantity of those elements.[1] In his *Erziehungslehre* he considers the pedagogical aspects of the subject.[2]

Among disciples of Herbart may be mentioned Drobisch, Volkmann, Dörpfeld, and Steinthal.[3] Of these, Dörpfeld emphasizes the comprehensiveness of memory, showing that all acts of thinking, comparing, and judging, and even the simplest perceptions of daily life involve memory; and as all reproduction is either immediate or mediate, as stated above, he attempts to reduce the laws of association to one law, and considers the important relation in which memory stands to thought. Steinthal considers the general principles of memory, and develops the Herbartian doctrine in harmony with his own theory of apperception.

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[1] See Lehrbuch d. Psych., Ch. III; also Die Neue Psych. and Psych. Skizzen.

[2] Bd. I, §§20–22.

[3] Cf. Drobisch's Empir. Psych., §§31–41 and 117; also his Mathemat. Psych., passim; Volkmann, Lehrbuch d. Psych., Bd. I, IV (this is especially valuable for many notes and references); Dörpfeld's Denken und Gedächtnis; Steinthal's Psych. und Sprachwissenschaft, Th. I, §§79–91 and passim. Also, for a good account of the views of Dörpfeld and Steinthal see Fauth's Das Gedächtnis: Gütersloh, 1888.

VI.—*Lotze.*

Lotze, among recent philosophers, is the most able champion of the doctrine that memory is a "power of the soul to preserve impressions independently of physical conditions, and to unite them according to laws that have nothing in common necessarily with the modes of action of nerve-forces." Even if the opposite theory of the dependence of memory upon physical processes be tenable, he would choose the former for ethical reasons; for in the next sentence to that quoted he says: "Where two hypotheses are equally possible, one harmonizing with moral needs, the other at variance with them, nothing can turn the choice in favor of the latter."[1] Yet Lotze does not deny the influence of cerebral changes on the train of thought. On the contrary, he maintains that there is an interaction between mind and body, that "not only the nervous stimulation conditions a definite presentation, but also the presentation, emerging again in the course of remembrance, reacts and strives to reproduce that nervous state by which it was itself aroused in sense-perception."[2] But the brain is not the organ of memory; for, while admitting the indirect dependence of the train of thought on bodily processes, he says: "The doctrine of a special organ of memory, even as a mere means of support to the soul's own power of remembrance, is exposed to greater difficulties than is commonly thought. The objection that the cerebral mass, which is not unalterable, but undergoes slow renovation, could not, without confusion, retain for future use the impressed copies of countless impressions, is met plausibly but not convincingly by reference to the countless undulatory movements of sound and of colored light that can simultaneously traverse the same atmospheric space without mutual disturbance."[1]

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[1] Medicin. Psych. §36, p. 473.

[2] Op. cit., p. 474.

The images of sense-perceptions, Lotze argues, are not strictly images. The mind takes cognizance only of qualitative differences of stimulation. Sense-impressions are but *intensive* data by the help of which the mind by its own activity reproduces the external world. To suppose that the brain retains impressions of the countless images of sense is possible only on the hypothesis that each cerebral atom is capable of retaining in itself without mutual disturbance an infinite number of impressions. "Such a theory would simply contain many repetitions of the same supposition that we make once. If every several atom of the cerebral mass is capable of retaining without confusion numberless impressions, why should the soul alone, like the atom a simple being, be incapable of doing so? Why should it alone not possess the faculty of memory and recollection in itself without the aid of a corporeal organ, when we have to concede that faculty directly and without the mediation of a new instrument to every part of the assumed organ? Nay, we must, in fact, make the contrary assertion, that the retention and reproduction of impressions is possible, not to a number of co-operant cerebral particles, but exclusively to the soul's undivided unity. . . . . to admit an organ of memory would only lead to our having to attribute a memory to the soul, and also to regard the several atoms of the brain as souls whose power of remembrance assists ours."[2]

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[1] Microcosmus, Book III, Ch. III, §5.

[2] Loc. cit.

Lotze, however, does not neglect empirical facts, but considers the relation of sense-perception to reproduction, and discusses the morbid phenomena of amnesia and hypermnesia, finding nothing that contradicts his theory of memory. His discussion of our subject is one of the ablest arguments in modern times for the transcendental theory of memory. Yet, as Horwicz says: "From this man we can learn to think upon psychological questions in terms of physiology."[1]

VII.—*Fechner.*

Fechner, in accordance with his Spinozistic principle of the double-aspect nature of the world, writes thus of memory: "Remembrances (*Erinnerungen*) are developed from perceptions, under the supposition of a universal consciousness in which both are included. Without knowing the psycho-physic processes which underlie the one or the other, we can, nevertheless, conclude according to the functioning principle, that the psycho-physic conditions of remembrances are developed out of those of perceptions, under the supposition of universal psycho-physic conditions which the nature of the universal consciousness demands."[2] These theoretic views we need not dwell upon here. Of special interest, however, are his observations in regard to the relation between after-images and memory-images.[3] According to him, the chief difference between the two consists in a feeling of *receptivity* connected with the former, in contrast with a greater or less feeling of *spontaneity* connected with the latter. He observed carefully the difference between memory-images and after-images in his own experience, and questioning others skilled in introspection—such men as Volkmann and Drobisch—found great personal differences in the vividness and distinctness of the memory-images.[1] At one end of the scale stood Fechner himself, with memory-images "incomparably less distinct" than after-images; at the other end the painter, reported by Boismont, who, in a half-hour's sitting, obtained such a vivid memory-image of his subject that it served him afterwards as a model.[2] The one distinction between memory-images and after-images found by all observers was, that while the former can be modified at will, the latter cannot be voluntarily changed.

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[1] Psych. Analysen, Bd. I, p. 272. For Lotze's discussion of the train of ideas and criticism of those who explain memory after the analogy of physical inertia (Fries et al.), and of the Herbartian theory, see Microcos., Book II, Ch. III, and Outlines of Psych., Ch. II. Cf. also Metaphysic, B. III, Ch. II and V.

[2] Elemente d. Psycho-physik, II, p. 380. I translate *Erinnerung* "remembrance" rather than "recollection" because the latter implies volition, while the German word as used here and below is not limited to voluntary recollection.

[3] Op. cit. II, p. 468 seq.

Fechner distinguishes five classes of phenomena in respect to persisting images: (1) after-images, *i. e.*, after-sensations; (2) memory-images; (3) memory-after-images; (4) phenomena of sense-memory; (5) involuntary hallucinations. By the third class—memory-after-images—Fechner means memory-pictures formed immediately after the perception, and having, therefore, a vividness comparable to that of the after-sensation. Under the fourth class he includes after-images that appear involuntarily some time after the original sensation.[1] These different phenomena form a sort of gradation between the sensory after-image on the one hand and the memory-image on the other. Fechner concludes from his observations that the processes underlying the phenomena of after-images and of memory-images are essentially the same. If the psycho-physic process is aroused within and passes outward, we have a memory-image; if it proceeds from without inward, we have an after-image.

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[1] Cf. also Galton's observations in regard to Mental Imagery, Enquiry into Human Faculty, p. 83 seq.

[2] The painter described his method of work after such a sitting in these words: "Je prennais l'homme dans mon esprit, je le mettais sur la chaise, où je l'apercevais aussi distinctement que s'il y eût été en réalité; et je puis même ajouter avec des formes et des couleurs plus arrêtées et plus vives. Je regardais de temps à autre la figure imaginaire, et je me mettais à peindre; je suspendais mon travail pour examiner la pose, absolument comme si l'original eût été devant moi; toutes les fois que je jetais les yeux sur la chaise, je voyais l'homme." Brierre de Boismont, Des halluc., p. 39, quoted by Fechner.

VIII.—*Horwicz.*

Horwicz makes an important contribution to our subject, by showing the relation of memory to feeling.[2] He distinguishes remembrance (*Erinnerung*) from mere memory (*Gedächtniss*), by limiting the former to cases where the time and space distinctions of the original presentation are revived, and using the latter to denote cases where only the content of the presentation is renewed. Considering the subject from a physiological standpoint, he shows that acts of reproduction are so involved in all psychic life, that the assumption of a special organ for memory is inconceivable. He would rather ascribe the power of reproduction in varying degree to the whole nervous system. We find the germs of memory even in simple reflex action. In case of the decapitated animal or the sleeping man, a futile movement for removing a stimulus is followed by a more appropriate one. This would be impossible

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[1] This phenomenon was first mentioned by Henle.

[2] See his Psych. Analysen, Bd. I, pp. 265–331; also pp. 160 seq., 256 seq., 369 seq.

without remembrance of the first movement and its ineffectiveness. In Pflüger's experiment, the segments of an eel's tail react *from* the flame, not into it. "Hence it follows that remembrance (*Erinnerung*) dwells in each central organ, yes, even in each smaller complex of nerve elements."[1] Yet we may regard the great commissures of the brain as the most important organs of reproduction. The manifold trains of commissural fibers seem specially adapted to function the linking of our ideas; and there is no other use to which they do seem adapted.

The assumption of this physical basis for remembrance does not, in the opinion of Horwicz, prove materialism; for many facts tend to show that these fibers were developed by psychic activity. Nor does the anatomical structure of the commissural system lend support to the Herbartian psychology; for there is no one point (corresponding to Herbart's statical point toward which all presentations strive) toward which all nervous paths converge.

If reproduction is a function of one class of the commissural fibers, *i. e.* those of the brain, then "reproduction is a special case of association." Taking the spinal cord as the simplest example of a central organ, its three classes of commissural fibers, the lateral (uniting sensory and motor cells and functioning simple reflex actions), anterior (connecting motor cells with each other and associating movements), and posterior (connecting sensory cells with each other and associating sense-impressions), afford a physiological explanation of the association of presentations.

This, however, does not explain the preservation of impressions in memory. The simple remembrance of a former stimulus seems to be confined to the same nerve-fiber and cell as the original impression. But this is not so. We fail to recognize at once known objects in unfamiliar places. The prick of a pin we recognize whether on the foot or on the hand. Therefore remembrance does not occur in the simplest case without association.

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[1] Op. cit., p. 278.

Moreover, every sensory nerve-path passes through several cells. Hence it is probable, Horwicz concludes, that the sensation and the remembered sensation do not have their seat in the same cells, but that some cells are devoted exclusively to memory. Probably in the higher central organs a memory-fiber, at every station of the afferent nerve-path, branches off and ends in a terminal cell. In these memory-cells we must assume that a trace of the sense-impression is deposited, which trace is associated with new impressions as soon as the stimulus strikes the neural path concerned. We must not, however, suppose that this storing-up process is something peculiar to the so-called memory-cells. From the identity of nerve-structure we must rather assume that the process is the same on the whole neural path. But in the conducting fibers and the intermediate cells, we may suppose that, on account of their frequent stimulation and other functions, the storing-up process cannot so easily occur, but only a disposition to definite associations remains. "The universal law of the association of nervous excitations appears at the first stage as an accessory sensation (*Mitempfindung*) and an accessory movement (*Mitbewegung*), in the course of the nerve-path as a disposition, at the end of it as an after-sensation. The after-sensation is related to remembrance as the accessory sensation and accessory movement to the association of sensations and movements."[1]

Thus reproduction is a kind of association, and association in its widest sense, *i. e.* the transmission of an excitation from one nerve-fiber to others, is a universal function of the central organ. Anatomical necessity conditions sensory and motor association originally, since the excitation must spread equally over all cells connected with the stimulated center. In this primary stage association appears as accessory movement or accessory sensation. Gradually, by chance or otherwise, the spread of the stimulus preponderates in one direction. This preponderance gradually increases until, finally, a decided inclination for a definite direction is formed. This transmission of a stimulus with a decided disposition is association in the narrowest or usual sense.

Leaving the motor side, and attending exclusively to the association of sensations, Horwicz gives the following definition as a résumé of what has been said: "*Reproduktion ist eine Uebertragung von Reizen überwiegend nach der sensiblen Seite in bestimmten durch Dispositionen angegebenen Richtungen und auf Residuen, die von älteren Reizzuständen aufbewahrt sind.*"[2]

Horwicz discusses the theory that explains memory after the analogy of the persistence of physical forces, and shows that the law of inertia cannot hold in the mental world in the same way as in the physical. In order really to understand the persistence that occurs in memory, we must understand the nature of sensation. The latter, however, is unknown. Therefore we can at most only investigate the relation of remembrance to sensation. It is apparently so easy to distinguish between sensation and remembrance that most psychologists have not thought it worth while to notice the distinguishing marks of the two. Fechner, however, gave special attention to this;[1] and the phenomena discussed by him show that remembrance is distinguished from sensation by the greater influence of the will in the former.

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[1] Op. cit., p. 289. It should be stated that Horwicz offers this explanation only as an hypothesis.

[2] Op. cit., p. 291.

The persistence of presentations is no mere mechanical one. If the volition is the essential mark that distinguishes the remembrance from sensation, then it is also the ground of the persistence of the presentation. The persisting presentation is itself volition, activity, impulse. A sensation is essentially an impulse to movement, consequently the remembrance must be such an impulse stored away in the nerve-element. We may conceive of this physiologically as if a spring were made tense by pressure. Then suppose that a second spring is so united with it that when the first is pressed the second is made tense also, but is not relaxed when the first is relaxed. Imagine an infinite number of such spring-systems, of which now these, now others are affected, and in the elasticity of the second springs we have a fitting illustration of the preservation of the sense stimuli in memory.

Horwicz considers the relation between the conscious and the unconscious in our mental life, and shows that the transition from the one state to the other is ordinarily a gradual one. All psychic activities divide into the two classes of conscious and unconscious processes. These two affect each other reciprocally, the conscious drawing forth the unconscious, and the unconscious pressing upon the conscious.

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[1] Cf. supra.

The original sensation, the perception of a nervous excitation, is an impulse that shows itself immediately by translation into motion. The sensation also remains such an impulse if, instead of the motor path, it strikes the memory-cells. We have proof of this in the fact that presentations as well as sensations can originate movements, and that even unconscious presentations are wont to have clearly motor effects (*e. g.* in the case of the instantaneous movements for retaining one's equilibrium called forth by the unconscious or weakly conscious idea of the circumstances of the situation). How does this impulse suddenly become latent, and again suddenly become active? Or, in other words, how does the presentation suddenly vanish, and again suddenly appear? We say that a presentation vanishes when the attention is turned to something else. But it is a poor excuse for letting the soup boil away, that the cook was sprinkling the roast. It is commonly supposed that the soul, on account of its unity, is capable of but one presentation at a time.[1] But the unity of the soul does not explain the facts of the unconscious. How can we conceive the latter in terms of physiology?

The physiological explanation is found in the mechanism of inhibition or arrest. The strength and life of the *feeling* connected with a presentation gives it the power to obscure others, just as it is the will (*i. e.* the result of feeling) that inhibits movements. The attention may be divided among indifferent presentations, but presentations highly colored with emotion thrust back all that resists them. We can easily note the feeling of effort that it costs to bring foreign ideas or sensations back to consciousness.

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[1] Cf. Herbart, supra.

On the other hand, the return of presentations to consciousness is not so simple a matter as the Herbartians suppose. According to them, the presentation flies up like the spring when the pressure is removed. But the incalculable kaleidoscopic interplay of our presentations does not seem to conform to any such simple rule of arithmetic. "Not simple inertia, mere continuance, is sufficient to explain the persistence of presentations, but a continuing impulse, a constant act of life. Not the mere entrance of a new (opposite or however otherwise related) presentation is sufficient to arrest the former, but there must proceed from the new presentation a voluntary act of inhibition. So also the mere removal of the inhibition is not sufficient to allow the arrested presentation to appear again in consciousness, but living, and probably voluntary, relations must prevail between the presentations that arouse each other. Living impulses must condition the coming and going of presentations."[1]

In the development of our knowledge of the external world, movement and sensations of movement play a most important role. "But movement is the immediate result of the impulse involved in the sensation, *i. e.* of feeling. Consequently, we have to look upon feeling as the proper basis of remembrance." As feeling conditions voluntary and involuntary attention, thus, according to Horwicz, feeling is the ground of the persistence of presentations and of voluntary and involuntary remembrance. But the ground of the persistence of a presentation is a continuing motor impulse, *i. e.* the impulse to respond to a definite stimulus with a definite movement.

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[1] Op. cit., p. 314.

If feeling is the vehicle of the train of thought, it may seem strange that it is so hard to remember feelings. But it is not feeling, as such, which is the primary element in remembrance, but feeling in its necessary union with the feeling of motion. We can remember a feeling to the extent that we can reproduce the movements involved in it. Feeling is not directly, but indirectly, the basis of association. "Hence is explained how, on the one hand, feelings can most powerfully control and condition the train of our presentations, and how likewise, on the other hand, it is so difficult, yes impossible, to reproduce a feeling in itself."

Horwicz considers also the laws of the association of ideas, and maintains that in the traditional laws handed down from Aristotle, contrast is omitted, but that it is easily explained by his theory of feeling. Further, he considers the relation of his theory to the unity of consciousness, and finds the possibility of that unity in the connection of the nerve-cells in every direction.

IX.—*Wundt.*

Wundt's[1] theory of memory may be briefly stated. According to him, memory in its widest sense as the general power to renew presentations is the prerequisite for imagination and intellect. It has both a physical and a psychic side. On the physical side, it has its ground in those changes of sensibility that facilitate the return of conditions of stimulation that have once been aroused. The reproduction of vanished presentations depends on persisting dispositions to these presentations. All the phenomena of habit and of adaptation to circumstances indicate that the residua of impressions are functional dispositions. But "all reproduction proceeds from the presentations which are in consciousness, and the presence of unconscious dispositions does not revive the presentations, unless the necessary conditions for the forming of associations are present in consciousness itself. . . . The dispositions unconsciously present, and the degree of their registration, determine only what presentations in general can enter consciousness, but the actual entrance of any given presentation is always occasioned by the condition of consciousness itself."[1] Thus on the psychic side, reproduction is conditioned by psychic stimulation; and the starting point of recollection is always the present content of consciousness.

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[1] See his Grundzüge d. Phys. Psych., Bd. II, Ch. 17, 2te Aufl.

X.—*Organic Memory.*

The modern theory of unconscious or organic memory began long before Hering's famous lecture before the Imperial Academy of Sciences, at Vienna, in 1870. The germs of the doctrine are found in Malebranche. This Cartesian, as we have seen, explained memory by supposing that, when the brain-fibers have once received an impression, they acquire a facility of receiving the same impression again, just as the branches of a tree that have been bent in a certain manner acquire a readiness to be bent afresh in the same manner. Thus we think of the same things when the same cerebral processes recur. Moreover, in Malebranche's opinion, not the brain alone acquires this disposition to repeat changes that have once occurred, but the same is true of all parts of the body. Thus he says that, apart from consciousness, there would be no difference between memory and the other habits of the body.[1]

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[1] Op. cit., p. 319.

That Malebranche uses the word "habit" in referring to the repetition of organic processes, instead of the phrase "organic" or "unconscious memory," does credit to his clear thinking and to his care in the use of language. The defects in Malebranche's psychology of memory were due to the crude condition of physiological science at his time; but the distinction that he made between *memory* and *habit*, making the latter the genus and the former the species, has been, I think, the prevailing one among physiologists ever since.

Moreover, among German writers, Jessen, in his *Versuch einer wissenschaftlichen Begründung der Psychologie*, published in 1855, expressed a somewhat obscure doctrine of organic memory.[2] In his opinion, memory could, in a certain sense, be attributed even to things without life; for solid bodies retain impressions made upon them.[3] In illustration of this he cited such facts as the following. Musical instruments that have always been correctly played are superior in purity of tone. It is impossible to get an entirely pure note from a flute that has been wrongly played. The particles of the wood reproduce the accustomed vibrations. In all living organisms thought appears as a force existing in matter. Why, he asks, should not the phenomena of thought be derived from thinking matter as well as the phenomena of electricity from electrical substances?

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[1] Cf. my preceding article, footnote 2, p. 68.

[2] Cf. Huber, Das Gedächtniss, p. 24; and Fauth, Das Gedächtniss, Ch. II.

[3] Cf. Draper, Conflict between Science and Religion, p. 132 seq.

According to him, thoughts arise in one part of the brain, and come to consciousness in another part. The higher mental activities are located in the cortex of the cerebrum. If a clear image of an absent person is perceived, an efferent process from the brain-ganglia to the retina of the eye must be reproduced. Memory is no abstract property of the mind, but a universal property of nerves. In some inexplicable way, every impression persists in the nerves. The oftener and more strongly impressions are repeated, the more permanent becomes the effect upon the nerves, and the more easily can the appropriate movements recur. The ideas persist, not in the nerve-fibers, but in the nerve-cells. Hence, the special seat of memory is in the gray columns of the spinal cord, in the gray matter of the cerebral ganglia, and in the cortex of the brain.

This theory of organic memory was first brought into prominence, however, by Hering's lecture "On Memory as a Universal Function of Organized Matter,"[1] delivered before the Imperial Academy of Sciences, at Vienna, May 30, 1870. Starting from the law of the functional interdependence of matter and consciousness, and recognizing the necessity of supplementing physiology by psychology, he attempts to regard, under a single aspect, a great number of phenomena belonging partly to the conscious, partly to the unconscious life of organized beings. This common attribute is the memory or power of reproduction in organized matter. Examining memory closely, we find it to be a power of our unconscious as well as of our conscious life. Ideas appear for a moment on the stage of conscious life, then vanish, and reappear to-morrow. Where have they been meanwhile? They do not exist as ideas. Between the "me" of yesterday and the "me" of to-day are vast abysses of unconsciousness. What is continuous is the disposition of nerve substance, in virtue of which it repeats to-day the processes of yesterday. Again, the train of our ideas seems often to disregard the order that would obtain in a series of cerebral processes. Here the organic processes are connected in an orderly manner, but not all the links of the series appear in consciousness. The bond of union, then, must be in the unconscious world; and as, "for purely experimental purposes, 'matter' and the 'unconscious' must be one and the same thing, so the physiologist has a full right to denote memory as, in the wider sense of the word, a function of brain substance whose results, it is true, fall, as regards one part of them, into the domain of consciousness, while another, and not less essential, part escapes unperceived as purely material processes."[1] Moreover, all the phenomena of habit and our automatic processes give repeated illustration of the power of reproducing organic processes without consciousness, due to the memory of the nervous system.

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[1] Ueber das Gedächtniss als eine algemeine Function der organischen Materie. I have been unable to obtain this in the German, so have been obliged to refer to Butler's translation. See Butler's "Unconscious Memory," Ch. VI.

The same power of reproduction is found in other kinds of organized matter. The more a muscle is used the stronger it becomes. We have here, in its simplest form, the same power of reproduction that, in a more complicated form, is found in nervous substance. The same is true in greater or less degree of all our organs. Increased use, alternating with periods of repose, gives the organ increased power of execution, increased power of assimilation, and a gain in size. Three things are involved in this: (1) internal alteration of the molecular disposition of the cells; (2) increase in the size of the individual cells; (3) multiplication of the number of cells. The last is a result of the other two; for, when cells have attained a certain size, they give rise to others that inherit more or less completely the qualities of the original cells.

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[1] Butler's translation, p. 111.

This same power of memory is seen, according to Hering, in the facts of heredity. Acquisitions made during the life of the individual organism are transmitted to its offspring. In some mysterious way, the nervous system, in spite of its thousandfold subdivision into cells and fibers, forms a united whole, and even each cell of the more important organs is intimately related with the whole. This seems to be especially true of those germs marked out for independent existence, and each of these germs bears its part in the activities of the whole organism. Hence the offspring inherits the acquired peculiarities of the parent, or rather remembers them; for the descent of these peculiarities is a reproduction by organized matter of processes in which it took part as a germ in the parent organism, and of which it seems still to retain a remembrance, since, on occasion of given stimuli, it responds to them as the parent organism responded to the same stimuli. Thus the facts of heredity are as wonderful as when a gray-haired man remembers the events of childhood, but no more so.

If the germ can reproduce characteristics acquired by the parent, all the more will it be able to reproduce those that have developed through countless generations in the organized matter of which the germ is a fragment.

"An organized being, therefore, stands before us a product of the unconscious memory of organized matter. . . . . Thus regarded, the development of one of the more highly organized animals represents a continuous series of organized recollections, concerning the past development of the great chain of living forms, the last link of which stands before us in the particular animal we may be considering. As a complicated perception may arise by means of a rapid and superficial reproduction of long and laboriously practised brain processes, so a germ, in the course of its development, hurries through a series of phases, hinting at them only."[1]

The memory of organized substance is especially seen in the phenomena of instinct. This power of remembering the experience of ancestors must be ascribed to the new-born infant as well as to animals. The brain processes upon which consciousness depends have had a less ancient origin than those relating to physical needs. Hunger and the reproductive instinct affected the oldest and simplest forms of organic life. Hence the memory of organized matter is strongest in respect to them.

Thus Hering finds the explanation of habit, instinct, and heredity in a phenomenon of consciousness, and, according to him, memory is a biological fact. "Man's conscious memory comes to an end at death, but the unconscious memory of Nature is true and ineradicable."

An interesting application of this theory to disease has been made by Dr. Creighton.[1] Basing his opinion upon the principle of Hering that memory is a function of all organized matter, he uses the word *memory* in no figurative sense when he says: "Embryonic development, growth, and the continuity of organic life are the actual and explicit manifestations of that memory which was potential, implicit (and known to exist by inference only) in the sperm-particles and egg; just as consciousness is the actual and explicit manifestation of that memory which is potential, implicit (and known to exist inferentially) in the vast reserve of the unconscious which is at any given moment behind the scenes."[2] Applying his principle to many chronic diseases, he finds them characterized by a reversion to older and lower modes of life. The catarrhs, for example, "are a return, for a short time, to a more elementary, primitive, or embryonic kind of epithelial function."[3] Hence the value of alteratives consists in their habit-breaking or memory-breaking action. An alterative "effaces the memory of morbid action by substituting an action like it."

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[1] Butler's translation, p. 125.

The terminology of those writers who use the phrase "organic memory" has justly been criticized. The phrase is at best a figure of speech, a kind of metonomy or synecdoche. All memory involves retention, and reproduction as well. But not all retention and reproduction involve memory. In the poverty of scientific language, the word *memory* is a very convenient term, and the extension of its meaning to include all retention of impressions by organic matter, and the functional disposition to repeat organic processes in general, has the advantage of showing the relation of memory proper to the physical processes that form in large part the subject-matter of physiology and biology, and it helps to unify in one concept many diverse phenomena—an economic device that is often valuable. But there are serious objections to such a use of the word *memory*. The idea of consciousness is so deeply involved in the connotation of the word that it should be restricted to its ordinary, limited use. And the extended use of the word is unnecessary; for what is this functional disposition to repeat organic processes but the law of habit? Habit and memory are intimately related, but the word *habit* is the broader term. To say that a diseased organ has lapsed into a bad habit is more in accord with custom than to say that it remembers an old and lower mode of functioning. To speak of the *habit* of a cell is not as confusing as to speak of *cell-memory*. The fact of the retention of impressions and the reproduction of physiological processes by organic matter, no one will deny; but "it is no more fitly called 'organic memory' than are the molecular alterations produced in the wood of an old Cremona."[1]

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[1] See his Unconscious Memory in Disease.

[2] P. 16.

[3] P. 37.

XI.—*Conclusion.*

The number of modern writers who have discussed the subject of memory is legion. It is impossible to mention them all here. But the writers whose views have been presented in this article are representative men, and their theories illustrate the conceptions of memory prevalent in their respective schools. The continued Platonic and Aristotelian influences may still be noticed in these modern theories; the former appearing especially in the transcendental conception of memory, which was taught by the German idealists, and appeared in modified form in the Scottish School, and later found its ablest champion in Lotze; the latter appearing more or less in the empirical conceptions of the Associationists, Herbartian as well as English, and in modern physical theories.

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[1] Prof. Ladd in Elements of Phys. Psych., p. 554.

Finally, it must be plain that, whatever be the relative merits of the idealistic and the physiological theories of memory, the facts of introspection have been pretty thoroughly worked over in the continued discussions of memory, from the days of Plato and Aristotle down to the last German student who has contributed a thesis *Zur Theorie der Reproduction.* After our historical orientation, the quarter of the horizon that looks most promising is in the direction of empirical study. In a future article I hope to notice briefly some recent views omitted in the present chapter, together with the empirical studies of Ribot, Kraepelin, Ebbinghaus and others, and to add the results of some experiments of my own.

PERSONAL EQUATION.[1]

EDMUND C. SANFORD, PH. D.

II.

VARIATIONS IN THE AMOUNT OF PERSONAL EQUATION.

A certain variability in the personal equation is generally admitted. Those that assert its constancy, like Wolf,[2] separate it into two factors, one depending on the circumstances of observation, the other on the observer, and mean that the last is practically constant. But even this must not be taken too strictly. There are changes of personal equation while the circumstances of observation so far as known remain the same—progressive for some observers; quite without order for others. Both are illustrated in the equations of Main, Rogerson and Henry at Greenwich from 1841 to 1853 (observations by eye and ear). From 1846 the calculations are given by the Astronomer Royal,[1] the earlier ones are supplied by Peters.[2]

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[1] Continued from AMERICAN JOURNAL OF PSYCHOLOGY, November, 1888. The last section of the first part dealt with the absolute personal equation, and the last paragraph but one with a method of getting the absolute personal equation from transits of real stars. The following wholly different method, applied by Bredichin (*Annales de l'Observatoire de Moscou*, Vol. II, part 2, page 69), may be mentioned here. He replaced the central wire by a plate of metal having its sides parallel to the other wires. He observed at the wires in the usual way and by immergences and emergences at the edges of the plate. The mean of the last two gives approximately the true time of the transit of the middle of the plate (within 0.02 s for Bredichin), from which the personal equation for the wires can be obtained.

[2] Wolf: *Annales de l'Obs. de Paris, Mémoires* VIII, 153. Hough: American Assoc. Proceedings, XVI, 22. Wolf admits changes through fatigue, etc.

	M—R	M—H
	s.	s.
1840	= —0.15	= ..
1841	= +0.08	= —0.09
1842	= ..	= —0.01
1843	= +0.20	= —0.02
1844	= +0.18	= —0.05
1845	= +0.20	= —0.12
1846	= +0.26	= —0.05
1847	= +0.35	= —0.03
1848	= +0.37	= —0.04
1849	= +0.39	= —0.05
1850	= +0.45	= —0.11
1851	= +0.47	= —0.11
1852	= +0.63	= —0.00
1853	= +0.70	= +0.03

The increase of M—R is said by Dunkin, a fellow observer, to be due to a change in Rogerson's method of observing, but he fails to specify the nature of the change.

Further evidence of variability is furnished by changes in the difference between the personal equation for eye and ear and chronographic observations. Hilfiker found differences in his own personal equation by the two methods as follows:[3] (1883) 0.074 s, (1884) 0.086 s, (1885) 0.053 s, (1887) 0.022 s. Those below are given for the Greenwich observers:[4]

Excess of Clock Correction by the Eye and Ear Method.

	HT.	AD.	T.	L.	H.
	s.	s.	s.	s.	s.
1885	—0.02	+0.06	+0.04	+0.10	+0.63
1886	+0.05	+0.08	+0.05	+0.12	+0.59
1887	+0.03	+0.05	+0.02	+0.12	+0.58

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[1] Greenwich Observations.

[2] *Astronomische Nachrichten*, XLIX, 1.

[3] *Astronomische Nachrichten*, No. 2815.

[4] *Observatory*, Jan. 1888, p. 95.

The personal equation varies even from day to day. To mention only two from many instances, I may refer to the comparisons before mentioned, of Nehus and Wolfers, and those of Hirsch and Plantamour.

The personal equation for the sun and moon is different from that for the stars; and that for the first limb of the sun and moon is different from that for the second.—The observation of star transits by both methods has been described. In observing transits of the sun and moon, it is the custom not to bisect the disk, but to observe the transits of the limbs, and from them to get the time for the center. Astronomers have generally had a different feeling in observing stars and limbs; it is one thing to estimate the distance of the edge of a large bright disk from the wire or to fix the contact, and quite another to do the same thing for a star. It is also more difficult to observe the advancing edge when the wires are faint from the glare, than the retreating edge when the wires are black lines on a bright surface.

The feeling of the astronomers is justified by the probable errors. Dr. Robinson,[1] observing at Armagh in 1830 by the eye and ear method, found for a transit of the sun's first limb a probable error of ± 0.116 s, for the second ± 0.087 s, for stars ± 0.097 s. Downing found[2] from a hundred complete transits of the sun at Greenwich in 1874 and 1875, as follows:

	I Limb.	II Limb.	Star.
	s.	s.	s.
Prob. error of transit of one wire,	± 0.072	± 0.063	± 0.051
Prob. error of complete transit,	± 0.024	± 0.021	± 0.017

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[1] Proceedings of the Royal Irish Academy, VII, 371.

[2] *Monthly Notices*, XXXVIII, 102.

Other errors are here involved, but not such as to affect the relative amounts.

Besides the difference in certainty, the presence of a real difference of personal equation for the two limbs was early recognized. In 1848 a large discordance was found in the tabular errors of the moon in right ascension as deduced from the altazimuth observations of Dunkin and those of H. Breen. From a year's observations of the first limb the difference amounted to 0.46 s, from those of the second to 0.30 s. In 1869 Dunkin investigated the subject on a basis of six years' observations of the moon with the altazimuth and transit circle, and in 1874, on a basis of ten years' transit observations of the sun.[1] Five years later Neison studied the transit observations of the moon from 1863 to 1876.[2] The results are all in substantial agreement, as shown by the following tables:

Observations on the Moon.

DUNKIN, 1869:

	Transit circle.			Altazimuth.	
	I Limb.	II Limb.		I Limb.	II Limb.
C—D	+0.034	+0.032	C—D	—0.047	—0.028
C—E	+0.112	+0.077	C—E	+0.104	+0.048
C—JC	+0.132	+0.038	C—JC	+0.207	+0.004

NEISON, 1879:

RELATIVE PERSONAL EQUATIONS.[3]

	I Limb.	Wt.	II Limb.	Wt.
C—D	=+0.012	4	+0.032	3
C—E	=+0.106	9	+0.065	7
C—JC	=+0.125	8	+0.038	6
C—L	=—0.034	4	—0.058	3
C—AD	=—0.007	2	—0.109	2
C—T	=—0.040	1	—0.093	½

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[1] *Monthly Notices*, XXIX, 259; XXXV, 91.

[2] *Monthly Notices*, XL, 75.

[3] The signs in this table are changed, to make comparison with the rest more easy.

ABSOLUTE PERSONAL EQUATIONS.

	I Limb.	II Limb.
	s. s.	s. s.
D	+0.025 ± 0.012	+0.004 ± 0.014
E	−0.060 ± 0.009	−0.041 ± 0.010
C	+0.045 ± 0.008	+0.026 ± 0.009
JC	−0.074 ± 0.010	−0.014 ± 0.012
L	+0.075 ± 0.015	+0.066 ± 0.020
AD	+0.006 ± 0.022	+0.132 ± 0.026
T	+0.063 ± 0.025	+0.183 ± 0.050

The numbers in the first table are really mutual differences, obtained from averages of the tabular errors of the moon as found by the different observers, and contain, especially those for the altazimuth, other errors than those of personal equation. Those in the second table are averages of the mean personal equations of the fourteen years, weighted for number and distribution of observations. Those of the third are calculated on the assumption that the personal equations compensate each other and that their sum equals zero. The results for A, D, and T are less certain, because of the small number of their observations.

Observations on the Sun.[1]

DUNKIN, 1874:

	I Limb.	II Limb.
C—D	+0.042	+0.002
C—E	+0.103	+0.019
C—JC	+0.150	−0.001
C—L	+0.016	−0.037
C—HC	+0.034	−0.126

The differences come out most strongly for most observers in transits of the first limb where the glare is most troublesome, but not for all. Dunkin suggests no explanation but a difference in the "methods of estimating by the eye the positions of the two limbs in transit as affected by irradiation."[1] Dr. Robinson also assigned irradiation as the cause, and by lessening it was able to lessen the error materially.

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[1] The last two personal equations are not in Dunkin's table, but are figured from other tables given.

The personal equation changes with the magnitude of the star.—An early notice of such a connection, perhaps the earliest, comes from the elder Struve.[2] About 1869, the attention of Argelander was called to a difference of 0.66 s in the right ascension of the minor planet Egeria as determined at Bonn and Leyden. An examination of the observations, and exclusion of other sources of error, reduced the possible causes to a different perception of the instant of transit for faint and bright stars. He had before suspected that he himself observed the transits of very faint stars differently from those of bright ones. To test his suspicion, he examined his observations of variable stars as far as the ninth magnitude, but without finding any appreciable difference. With still fainter stars, 9.2 and 9.3 magnitudes, he found a slight tendency to observe too early, but not more for the latter than 0.15 s. Since this was insufficient to account for the difference in the position of Egeria, he suggested a variation of the same kind in the observatory of Leyden, where the observer might have been less experienced.

It was, indeed, at that observatory, though several years later, that the question was first thoroughly investigated.[3] In connection with a study of the orbit of Mars by Gill, in 1877, the place of a number of stars was fixed at the same time by twelve important observatories in different parts of the world. The resulting right ascensions showed differences that Gill traced, in part at least, to variations of personal equation with magnitude. Following such a suggestion, previously made to him by Gill, H. G. v. d. Sande Bakhuyzen, of the observatory of Leyden, reviewed the results. On the assumption that the mean of the right ascensions from the ten observatories (two were omitted for special reasons) was exact, he found the differences of each from the mean and plotted curves, to show the effect in each of a change of brightness in the star.[1] The form of these curves is distinct for the different observatories, and very interesting, but on a narrow numerical basis.

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[1] Introduction to Armagh Catalogue.

[2] *Positiones Mediae*, lxiv.

[3] In the same year as Argelander's demonstration of the difference, the question was touched experimentally by Rogers (*American Journal of Science and Arts*, Second series, Vol. XLVII, p. 297). His experiments dealt with several other causes of variation than the size of the star, and though, in comparison with later experiments, his are crude and, as the author himself recognizes, not

To put the question to a direct experimental test, Bakhuyzen and his assistants observed a number of transits, using for half of each the full objective, and,

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definitive, they will be referred to several times in what follows—a word, therefore, as to the manner of them. The artificial stars were made of paper and mounted on wires attached to a chronograph drum. Their transits behind a fixed wire and the record of the observer were taken electrically on the same drum. This record was measured to tenths and estimated to hundredths of a second. In general, eight stars were used, and ten turns of the drum made a series. In his experiments on magnitude, he and his assistant observed the transits of five paper stars, the first of which was larger than the rest. The result, in the form of relative personal equations, was: For the large star, R—T = —0.233 s; for other stars, R—T = —0.152 s; difference, 0.081 s. To show the same result in another way: the positive excess of T's personal equation for the large star over the small ones was 0.047 s; B's negative excess was 0.036 s; the difference of R and T, 0.083 s. No such clear difference could be traced in the observation of real stars. The experiments evidently do not touch the question of brightness apart from apparent area of the disk.

[1] These curves accompany Gill's article, ***Monthly Notices***, XXXIX (1879), 98, which is of later date than Bakhuyzen's communication in the ***Astronomische Nachrichten***, XCV, 187. Gill touches the question also in the same volume of the ***Monthly Notices***, p. 434.

for the other half, the objective reduced by a diaphragm to $\frac{1}{7.87}$. This reduced the stars about 2.3 magnitudes. To avoid errors, the diaphragm was used alternately for the first and second halves. These are the mean results for observations by the chronograph method :

REDUCED MINUS FULL OBJECTIVE.

	First half transit reduced.	No. of stars.	Second half transit reduced.	No. of stars.
E. F. Bakhuyzen,	+ 0.071	8	+ 0.070	8
Wilterdink,	+ 0.049	11	+ 0.047	8
Stieltjes,	+ 0.060	8	+ 0.034	9

Continuing his experiments, Bakhuyzen later published experiments with both methods of observing, in part of which the diaphragm was used, and in part a grating of fine copper wire that reduced the stars 2.8 magnitudes.[1] The following are the mean results :[2]

REDUCED MINUS FULL OBJECTIVE.

Chronograph Method.

	s. s.	Mean mag.	No. of obs.
E. F. Bakhuyzen,	0.049 ± 0.007	5.5	80
	0.045 ± 0.013	3.1	14
Wilterdink,	0.039 ± 0.009	5.3	48
	0.035 ± 0.019	2.5	5
Stieltjes,	0.041 ± 0.008	5.3	65
	0.045 ± 0.012	3.1	15

Eye and Ear Method.

	s. s.	Mean mag.	No. of obs.
E. F. Bakhuyzen,	0.054 ± 0.013	5.1	41
	0.065 ± 0.023	3.0	8
Wilterdink,	0.023 ± 0.020	5.4	16
	0.128 ± 0.033	2.1	4
Stieltjes,	0.041 ± 0.014	5.5	37
	0.103 ± 0.037	3.5	3

These tables show, in brief, that all the observers at Leyden observed bright stars too early by both methods.

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[1] *Vierteljahrssch. d. Astronom. Gesell.* XIV, 408.

[2] The mean magnitudes in the table are for full objective.

This increase in personal equation as magnitude diminishes probably has more than one cause. Wundt sees in it a close parallel of the increase in the length of the simple reaction time when the stimulus is diminished.[1] That is due to a change from the motor form of reacting, in which the attention is centered on executing the motion of recording, to the sensory form, where it is centered on catching the phenomenon to be recorded. Indeed, he suggests this kind of change as a general explanation of the change of relative personal equation. A change of attention might also affect observations with the eye and ear, as well as those with the chronograph.[2] In some cases this further cause also co-operates. It was explained in the first part of this paper that some observers, in using the chronograph method, anticipate the actual transit. They tap the key so as to make the sound or the muscular sensation of the closing coincide with the bisection.[3] If an observer does so, "he will be nearly certain," says Gill, "to press too soon for very bright, and too late for very faint stars, because the larger disk and the rings of a bright star will make it appear closer to the web than the small, sharp disk of a faint star, at the same angular distance from the web."

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[1] *Physiologische Psychologie*, 3d ed., II, 273.

[2] Compare the observations of Hartmann, mentioned in the first part of this paper, p. 29.

[3] This way of observing, if not the best astronomically, is no doubt the natural one—a special case of the general tendency that makes man forethoughtful. All the persons tested by Mitchel (first part, p. 27) anticipated. That it is common among astronomers also, may be argued from the size of the personal equations. The shortest reaction-times found by psycho-physical experimenters for recording the reception of an instantaneous and expected stimulus (a sound or a certain position of a pointer on a dial) are about 0.075 s. It is clear, therefore, that observers whose records follow the actual bisection by less than about 0.075 s begin the motion of recording before the phenomenon has actually appeared. Personal equations by the chronograph method of less than 0.075 s are, I believe, by no means exceptional.

Yet another cause is suggested by Newcomb.[1] In comparing the right ascensions given by observations on the transit circle and transit, he found that those given by the former for faint stars were too great. After explaining that the spider lines appeared finer and the field less brightly illuminated in that instrument, he continues: "The effect of the latter cause will probably be to make an observer later in recording a transit. . . . The stray light which surrounds a bright star forms a bright ground for the dark transit wire a perceptible time before the star reaches the wire, and thus the approach of the two objects is distinctly seen. As we take fainter stars the stray light disappears and the approach is less distinctly seen. Thus the effect in question will be exaggerated as the star grows fainter."

The personal equation is influenced by the direction of the star's motion.—I have already mentioned, in summarizing Wolf's experiments (first part, page 36), that his personal equation was 0.04 s greater when the star moved from left to right than from right to left. In a single experiment, where he lay down at right angles to his telescope and thus observed with the star moving along the vertical meridian of his eye, his personal equation was 0.127 s, against 0.118 in the ordinary position. Kaiser found (first part, page 32) only one of the four observers at Leyden who was affected by change of horizontal direction, though all four were affected by change from a horizontal to a vertical direction. About the same time (1867), in a treatise presented to the Austrian Academy of Sciences, K. von Littrow discussed the differences of personal equation in the east and west positions of the "broken transit" or reflection theodolite.[1]

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[1] Observations at the United States Naval Observatory, XIV (1867), Appendix III, p. 27.

In this instrument, to facilitate observation and give greater steadiness, the rays of light gathered by its objective are bent at right angles by a prism at the level of its transit axis, where one of the trunnions is made to do duty for the ocular end of the tube. In an ordinary transit instrument, stars south of the zenith appear to move from right to left, between the zenith and the pole from left to right, and below the pole again from right to left. Zenith stars themselves may be observed with motion in either direction according to which side of the instrument the observer places himself. In the "broken transit," however, the apparent motions are vertical or oblique. Suppose such an instrument set so that the telescope tube moves in the plane of the meridian, and the trunnion eye-piece points east. The observer then faces west, and without changing his position can sweep the whole of his meridian from the northern to the southern horizon. He can do the same thing when the instrument is turned around and he faces east. But there is this difference: when the observer faces west, all stars south of the pole seem to move upward in his field of view; zenith stars directly upward, stars between the zenith and the pole upward from left to right, those south of the zenith upward from right to left, and stars below the pole obliquely downward from right to left. When the observer faces east the direction of motion is reversed: all stars south of the pole seem to move downward, those between the zenith and the pole obliquely from left to right, south of the zenith obliquely from right to left, and below the pole obliquely upward from right to left. Observations on such an instrument are, therefore, admirably suited to bring out any differences that depend on direction of motion.

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[1] *Astronomische Nachrichten*, LXVIII, 369.

Differences of clock corrections found in the two positions of the instrument were noticed in 1864 by Weiss; and in 1865, differences between the personal equations of Föster and Weiss in the two positions were also noticed. The latter then calculated from his earlier figures, making certain assumptions, the difference of personal equation in the two positions. The amounts are somewhat irregular, but the following are the means found:[1]

OBSERVER EAST MINUS OBSERVER WEST.

	Weiss. s.	Bruhns. s.
Eye and ear,	—0.166	+0.072
Chronograph,	—0.214	—0.099

The observations on which these figures rest were of stars at about the same distance from the pole, and, therefore, cannot show the changes that very likely enter when that distance is changed, and with it the rate and obliquity of motion.

Littrow found further proof of the reality of the difference introduced by the direction of motion, in the tests made at Greenwich in 1852 and 1853 with the "binocular eye-piece" (first part, page 22), but never before worked up. To observers comparing for personal equation by means of this eye-piece, the stars would appear to move in the same direction, right and left, as with the ordinary eye-piece ; but to the one at the east branch, stars south of the pole would appear to move obliquely upward, to the observer at the west branch obliquely downward ;[1] for stars below the pole the directions would be reversed. From Littrow's tabulated results (observations by eye and ear only) I take a few for illustration :

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[1] Bredichin found by his method for absolute personal equation from real stars the following values for his personal equation in the two positions: observer east, first series —0.004, second series —0.015; observer west, first series 0.195, second series 0.241. The observations were by eye and ear on equatorial stars.

Hilfiker found the difference between his personal equation by the two methods to be different in the two positions of the instrument: observer east 0.024 s, observer west 0.018 s. For experiments incidentally touching direction of motion see also Buccola: *La legge del tempo*, p. 162.

D — E (Ordinary Personal Equation — 0.07).

	D east. s.	Stars.	D west. s.	Stars.
1853—Oct. 14	0.00	3	—0.21	3
20	—0.01	3	—0.17	3
Nov. 9	—0.24	3	—0.56	3

H — D (Ordinary Personal Equation + 0.13).

	H east. s.		H west. s.	
1852—Feb. 10	+0.27	3	+0.15	3
12	+0.28	2	—0.04	2
Mar. 12	+0.07	3	+0.11	3

R — D (Ordinary Personal Equation + 0.50).

	D east. s.		D west. s.	
1852—Jan. 23	—0.69	4	—0.52	4
April 2	—0.89	3	—0.47	3
26	—0.65	3	—0.21	3

In 1870 and 1871 Wagner incidentally made determinations with motions toward the right and left in studying his absolute personal equation with a Kaiser machine.[2] His experiments with the two directions were unfortunately not made at the same time and do not show constant differences. The figures, however, are given below in the section on the effect of rate (page 288) and may there be consulted.

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[1] Zenith stars would of course seem to move straight up and down.

[2] *Observations de Poulkova*, Vol. XII.

A few years later Campbell and Heaviside, in connection with longitude work in India, also found differences of personal equation depending on the direction of motion.[1] Their longitude observations were made by the chronographic method, and with an eye-piece that showed the stars in their actual motions —that is, stars north of the zenith with motion from right to left, and stars south with motion from left to right. The relative personal equation for the first was C — H = 0.124 s ± 0.0087 s; for the second, C — H = 0.040 s ± 0.0034 s, Heaviside observing a little earlier than Campbell. From twenty-one stars near the zenith observed by Campbell through half their transit as north stars and through the other half as south stars (the order being reversed, of course, to exclude error), the following equation results: N — S = 0.077 s ± 0.0067 s, motion from left to right being observed a little earlier than that in the contrary direction.

The most recently published and most satisfactory investigation of this question, though confined to motions left and right in a horizontal direction, was also made at the Greenwich Observatory.[2] An artificial transit apparatus on the plan of C. Wolf's, but improved at many points, was set up in 1885, and in 1887 the regular transit observers made special determinations of absolute personal equation with it for the effect of rate and direction. From the report of these by H. Turner I quote the following table. All the observations were by the chronographic method:

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[1] *Monthly Notices*, XXXVII (1877), 283.

[2] *Monthly Notices*, XLVIII, 4.

ABSOLUTE PERSONAL EQUATIONS WITH THE GREENWICH PERSONAL EQUATION MACHINE.

Observer.	Direction.	Rate one half as fast again as the equatorial.	No. of transits.	Equatorial rate.	No. of transits.	Rate at N. P. D. 43⅓°.	No. of transits.	Rate at N. P. D. 30°.	No. of transits.	Rate at N. P. D. 15°.	No. of transits.
		s. s.		s. s.		s. s.		s. s.		s. s.	
Turner........	l.	−0.053 ± 0.013	6	−0.065 ± 0.015	6	−0.098 ± 0.020	5	−0.115 ± 0.028	4		
	r.	−0.063 ± 0.013	6	−0.043 ± 0.015	7	−0.048 ± 0.020	5	−0.045 ± 0.028	4		
Downing......	l.	+0.094 ± 0.007	18	+0.064 ± 0.008	19	+0.045 ± 0.011	15	+0.036 ± 0.014	15	+0.052 ± 0.026	13
	r.	+0.072 ± 0.007	17	+0.075 ± 0.008	19	+0.065 ± 0.011	15	+0.076 ± 0.015	14	+0.137 ± 0.026	13
Thackeray....	l.	−0.042 ± 0.006	26	−0.077 ± 0.007	25	−0.108 ± 0.009	22	−0.158 ± 0.014	17	−0.109 ± 0.024	15
	r.	−0.044 ± 0.006	26	−0.036 ± 0.007	25	−0.062 ± 0.009	22	−0.069 ± 0.014	18	−0.019 ± 0.024	15
Lewis........	l.	+0.020 ± 0.006	25	+0.003 ± 0.007	22	+0.003 ± 0.010	20	−0.008 ± 0.012	21	−0.077 ± 0.022	18
	r.	+0.013 ± 0.006	24	+0.021 ± 0.008	20	+0.014 ± 0.010	20	+0.036 ± 0.013	20	+0.093 ± 0.022	17
Hollis........	l.	+0.221 ± 0.007	18	+0.256 ± 0.007	23	+0.260 ± 0.012	14	+0.316 ± 0.013	19	+0.457 ± 0.023	16
	r.	+0.234 ± 0.007	19	+0.267 ± 0.007	23	+0.274 ± 0.012	14	+0.349 ± 0.013	19	+0.528 ± 0.024	15

In this table *l* and *r* stand for left and right, the side toward which the motion took place. The plus sign indicates that the observation was too late.

It will be noticed that the first and third observers have personal equations with minus values, that is, they anticipate the true time of the transit by an amount greater than their simple reaction-times. Compared with that standard, the second and fourth also anticipate, but not enough to make the values of their equations minus. The fifth waits till he has seen the star actually bisected or a little past bisection. But all are alike in observing at the equatorial and slower rates a little later with motion in the less accustomed direction, namely, toward the right; the anticipators anticipate less, and the tardy one is a little more behind. The differences, however, are not very certain till the slowest rates are reached.

The same tendency is shown in observations on real stars. Since 1884 a portion of the clock stars have been observed at Greenwich with motion from left to right by means of a prism eye-piece. From these and altazimuth observations the following comparative tables are composed:

PERSONAL EQUATION WITH MOTION *r* MINUS PERSONAL EQUATION WITH MOTION *l*.

Stars at a little less than equatorial rate.[1]

	Artificial stars.	Real stars.	No. of nights.
H T	+0.027 ± 0.020	−0.035 ± 0.011	12
A D	+0.014 ± 0.012	+0.010 ± 0.004	70
T	+0.042 ± 0.010	+0.063 ± 0.012	28
L	+0.017 ± 0.011	+0.020 ± 0.004	72
H	+0.011 ± 0.011	+0.010 ± 0.004	85

Stars at rate of N. P. D. 43¼°.

	Artificial stars.	Real stars (altazimuth).
H T	+0.050 ± 0.022	+0.041 ± 0.020
A D	+0.020 ± 0.011	+0.030 ± 0.012
T	+0.046 ± 0.010	+0.039 ± 0.017
L	+0.011 ± 0.010	+0.047 ± 0.013
H	+0.014 ± 0.011	−0.015 ± 0.009

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[1] The column marked "No. of nights" gives the number of occasions on which stars were observed with motion from left to right; it is the custom to observe two stars every fine night in this way.

The experimental results and those with real stars show but little variation.

C. Wolf explains the difference of personal equation in eye and ear observations as a result of asymmetry in the eye, and substantiates it in his own case by experiment. Such a peculiarity in structure is possibly not uncommon. With the chronograph method there is little chance for the operation of such a cause. It might, however, come in, for those that anticipate, in making their estimation of the distance of the star from the wire, but no explanation is yet put forward.

Personal equation is influenced by the rate of the star.—I have already spoken of the experiments of Bessel and C. Wolf in this particular (first part, pages 13 and 37). Bessel's results were negative; Wolf's are summarized in the following table :[1]

Rate.	P. E.	Error of P. E.
s.	s.	s.
31.5	+0.141	0.014
40.3	+0.120	0.010
53.1	+0.108	0.012
86.9	+0.091	0.015

The personal equation decreases as the length of time required to traverse the reticle increases, that is, as the rate of the star decreases; but the uncertainty is least at a rate somewhat greater than the equatorial, and increases with change in either direction.

The study of Wagner mentioned in the preceding section was designed chiefly to bring out the effect of very slow rates and the difference, under those circumstances, of personal equation by the two methods of observation. The motion of the artificial star was from left to right in the first set of experiments, from right to left in the second. The following tables are abstracted from those that summarize his results;[1] the rates are indicated by the declinations to which they corresponded:

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[1] The rate is given by the number of seconds required to cross the reticle; the equatorial rate corresponded to a time of sixty seconds. The four rates are averages of groups; not the identical rates at which observations were made. It should be remembered also that all of Wolf's experiments were made by eye and ear.

FIRST SERIES, 1870.

Declination.	Absolute P. E. by eye and ear.	No. of series.	Absolute P. E. by chronograph.	No. of series.	Eye and ear minus chronographic.
°	s.		s.		s.
17.0	+ 0.010	3	+ 0.090	3⅓	— 0.080
40.1	— 0.009	5⅓	+ 0.061	7⅓	— 0.070
69.4	— 0.025	8	+ 0.070	7⅓	— 0.095
77.9	+ 0.086	6	+ 0.173	7	— 0.087
85 52′	+ 0.03	3	+ 0.14	3	— 0.11
86 27	+ 0.04	7	+ 0.31	7	— 0.27
88 22	— 0.20	7⅓	+ 0.32	7⅓	— 0.52
89 1	— 0.37	7	+ 0.17	7	— 0.54

SECOND SERIES, 1871.

Declination.	Absolute P. E. by eye and ear.	No. of series.	Absolute P. E. by chronograph.	No. of series.	Eye and ear minus chronographic.
30.4	+ 0.057	7	+ 0.065	7½	— 0.008
66.9	+ 0.029	8	+ 0.085	9	— 0.056
77.6	+ 0.002	8	+ 0.111	8	— 0.109
85 58′	— 0.06	7½	+ 0.29	7½	— 0.35
88 12	— 0.13	8	+ 0.33	8	— 0.46
89 1	— 0.40	8	+ 0.11	8	— 0.51

The personal equation with the chronographic method increases as the rate decreases, except for the fastest rate in the first set, where the trials were relatively few, and for the slowest rate of all. There is also an increasing tendency to anticipate in the eye and ear values of the second set, and the difference between the two methods is in both an increasing one. Results of the same general import as these by experiment were obtained from observation of actual stars. It should not be forgotten, however, that most of these rates are extremely slow (the bisection here must have continued an appreciable time), and the results are therefore not strictly comparable with those for more rapid rates.

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[1] In Wagner's tables the minus sign indicated an observation later than the phenomenon; the signs have here been changed throughout to facilitate comparison.

The latest experiments on this point, as on the last, are those at Greenwich, but they are not comparable with those of Wolf, because made by the chronographic method. The table on page 285 shows the effect of rate as well as of direction; there is little need of further comment upon them. For the first four observers the tendency to change is strongest when the motion is toward the left. The change is an increase in anticipation; the minus values become greater, the plus values less. With motion toward the right there is greater irregularity, but at the very slowest rate a tendency to delay. The last observer suffers an increase of personal equation with both directions of motion. Turner comments on the result of the study as follows: "There does not appear to be any general law representing the change of personal equation with rate. We might suppose the personal equation to be made up partly of error of bisection and partly of a constant error in time either deliberate or unconscious. We should expect the latter not to vary with the rate or direction of motion, and the former to vary inversely as the rate, and to change sign with the direction of motion. This is fulfilled approximately for all the observers except H, whose 'error of bisection,' amounting to nearly 1".0, does not change sign with the direction of motion, so that in this case the observer apparently waits until the star has traveled about 1" past the wire—whatever be the direction of motion—and then 'hangs fire' for about 0.2 s before the record is complete. Such a well-marked exception rather precludes generalization."

Without attempting to give an explanation where Turner has declined, it may not be amiss to notice a single suggestive point. Gould[1] has remarked on the difficulty that those who anticipate must have in adjusting the amount of their anticipation to the changing rates of the stars. If they record for a slow moving star when it is as far from bisection as a rapid one would have to be, the amount of their anticipation expressed in time is increased. Something of the same sort, but of a contrary effect, would trouble those that allow the star to pass beyond the wire before recording. A slow-moving star would take longer in reaching the point at which they begin to record than a rapid one. Four of these observers anticipate, as we have said, and one delays beyond the bisection. Correspondingly, in the table we find, at least for the direction of most customary motion, an increase of personal equation with decreasing rates.

So the question stands. The fact that personal equation is affected by the rate is established, but not yet the law nor the manner of its influence.

The personal equation is affected by yet other circumstances of observation.—The preceding sections have been concerned with the influence of the character of the object; in this, I shall speak of several other factors—the position of the observer, the illumination of the reticle, the kind of key used, and the like.

In actual observation, the position of the observer changes with the declination, and so with the rate, of the star. It has therefore been felt by astronomers that experiments on the effect of rate made, as in most cases, without change of position, did not fulfill the conditions. The point has, however, received some attention from the experimenters. C. Wolf[1] adapted his machine to observing when flat on the back, with the following result (series one and four were made in the normal position, for the sake of comparison; all were made by eye and ear): 0.113 s, 0.114 s, 0.125 s, 0.103 s. He thinks, however, that the difference does not prove a change depending on position, probably because his results were exact only to 0.01 s. Rogers[2] found that a very constrained position made his observations (chronographic) a little earlier (0.011 s in the mean) and, strange to say, somewhat more regular.

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[1] Cited by Gill from Gould's Transatlantic Longitudes.

In 1879, Bakhuyzen suggested an apparatus for the observation of artificial transits in the positions of natural ones,[3] and in the past year Wislicenus, of the Observatory of Strasburg, has reported such observations made with an ingenious device of his own.[4] His absolute personal equation in the five positions is as follows:

Rate expressed as declination.	Zenith.	45°	Horizon.	−45°	Nadir.
	s.	s.	s.	s.	s.
11⅓°	−0.19	−0.18	−0.21	−0.13	+0.14
60	−0.01	−0.02	−0.05	+0.04	+0.50
80	+0.52	+0.48	+0.32	+0.41	+1.77

The amount, and especially the direction of the illumination of the field and the reticle have been found important for personal equation. C. Wolf thought the illumination of the field of little effect, but Newcomb enumerates the brighter field as one among other reasons that might co-operate to make day observations systematically earlier than those at night. Rogers found that he observed a few hundredths of a second later when the wires were faint than when they were bright.

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[1] Loc. cit.

[2] Loc. cit.

[3] Vierteljahrssch. d. Astr. Gesell., XIV, 414.

[4] Noted in *The Observatory*, December, 1888, p. 443, where the following criticism is made: "It must be remembered, however, that in different positions of the telescope, the contacts of the recording apparatus [which moves with the telescope] might be sensibly different. Very little information is given as to the adjustment of this apparatus."

R. Wolf, of Zürich, with his assistant, undertook a careful investigation of the effect of direction of illumination.[1] He was led to this by finding his relative personal equation with Hirsch to be of different sign and amount when determined at Zürich and at Neuchâtel. It was discovered that the direction was without influence so long as the position of the ocular exactly suited the eye of the observer. But if the ocular were drawn out or pushed in too far, the sign of the personal error changed with the side of the illumination, illumination of the field and of the reticle having contrary effects, and the amount (which could reach several tenths of a second) varied with the amount of displacement of the ocular. These differences were not found with double-sided nor with day illumination. In circumstances described in a later communication,[2] two images of the reticle could be seen, which changed their relative positions right and left as the direction of illumination changed and as the ocular was displaced inward or outward. These briefly mentioned papers, though having to do with instrumental conditions, are extremely important for all determinations of relative personal equation where the sources of error they point out have not been excluded. Perhaps in some such way as this the enormous difference between Bessel and Argelander, and between some other pairs of astronomers, and likewise the progressive change of relative personal equation, are to be in part accounted for.

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[1] *Astronomische Nachrichten*, LXXV, 71.

[2] *Astronomische Nachrichten*, LXXVI, 369.

Personal equation is somewhat affected by the kind of key used in recording. Schiff reacted to an electrical stimulus on the tip of the tongue by means of a telegraph key, an "astronomical key," and a "book key," with these results: telegraph key, 0.156 s ± 0.023; "astronomical key," 0.164 s ± 0.016; "book key," 0.130 s ± 0.010. Tacchini found figures from observations on a Wolf artificial transit apparatus as follows: "astronomical key," 0.172 s ± 0.027; "book key," 0.166 s ± 0.016.[1] The key used in the Naval Observatory at Washington is something like a telegraph key, but of course held in the hand in observing. A small but genuine difference of personal equation is introduced if observers hold the finger at different heights above the knob when ready to make a record.

The study of Rogers included many circumstances besides those mentioned. Some of his results may be briefly mentioned here. A change of more than forty degrees in temperature, carrying the mercury below zero, produced no definite effect on his own observations, though a single set on each of two other observers showed a delay of 0.073 s and 0.107 s respectively. Fatigue had little effect, but records taken after sleep following fatigue were somewhat quicker. Sets taken at intervals during a fast of thirty hours were, on the average, 0.032 s quicker than a normal set taken just before. The relative personal equation between Rogers and one of the other observers for a set of stars separated by from ten to fifteen seconds was 0.069 s greater than for a set separated by only two or three seconds. The relative personal equation for the same two observers was 0.023 s smaller for artificial than for natural stars.

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[1] Tacchini: Sulla equazione personale. *Rivista sicula di scienze, lettere ed arti*. Anno I, Vol. 2°, p. 382, cited by Buccola, La legge del tempo, p. 170. The "astronomical key" was a button held between the first two fingers and pressed with the thumb; the "box or book key" (*tasto a scatola od a libro*) of Secchi is pressed with all four fingers.

The personal equation is influenced by circumstances more or less purely psychic.—The time of the perception of one sensation is affected by the regular recurrence of a second. And this is true not only in the eye and ear observations, where attention must be given to two, but also in the chronographic, where the second is merely accidental. H. Leitzmann discovered such an effect in chronographic records made within hearing of a loud ticking clock.[1] The seconds given by the clock were alternately too long and too short, the long being 1.05 s, the short 0.95 s. The tick that marked the beginning of each was also followed at about 0.35 s by another made with the forward motion of the second hand. Beginning with the first tick of the long second, the times were as follows: 0.00 s, 0.35 s, 1.05 s, 1.40 s.

Leitzmann was an anticipator, trying, as he says, "to make the feeling of movement that corresponds to the closing of the electric circuit in the key contemporaneous with the perception of the coincidence of the star and wire." In observing, he was accustomed, in addition to taking the transits of the vertical wires, to take also the star's declination by tapping his key when the star was midway between a pair of horizontal wires. In working up his results after the observations were complete, he found that those for declination grouped themselves on certain tenths of the second. In 129 observations the distribution was as follows:

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[1] Wundt's *Philos. Studien*, Bd. V, H. 1, S. 56.

Tenth of the second,	0	1	2	3	4	5	6	7	8	9
Distribution of obs.,	2	4	3	30	60	18	4	0	4	4

The last two-thirds of the time covered eighty-six observations; seventy-six of them fell on 3 and 4, nine on 5, and one on 1. A connection with the clock beat was evident, and the observer went on to examine the more accurate transit observations for traces of a similar disturbance. Such he was able to find.

Assuming that the differences that remained after reducing the observations at the individual wires to the central one were due chiefly to the disturbance in question, he distributed the amounts so found in groups corresponding to each fiftieth of a second, averaged them, and plotted curves from the averages. In his curves, plus ordinates correspond to an increase of personal equation; minus, to a decrease. The curves are somewhat irregular, but a typical one would show a short wave followed by a longer one, in each of which there is first a descending and then an ascending phase. The first wave would cover the first four tenths of the second, the other the remainder. The points of greatest personal equation fall near the beginning and end of the curve, those of least in the middle of the second portion. The variations that the curves represent amount at the greatest to about 0.054 s, and the mean probable error of each value to about 0.004 s. Leitzmann attributes the changes to the influence of the intruding stimulus on attention, which influence was undoubtedly strengthened in his own case by a habit of semi-consciously counting the beats as in observing by the other method. Into the details of his exposition, some of which seem to me open to criticism, I shall not enter here.

In observations by eye and ear, many observers show a preference for some special tenth of the second, so that it recurs in their recorded observations far more frequently than it should by chance, and this "mental time scale" is often unchanged by time or circumstance.[1] Whether this is to be attributed to slip-shod habits of estimation or to some untraceable association like those that give numbers and letters peculiar characters in the minds of some people, or to some effect of the clock-beat like that just described, it is impossible to say without fuller information. Similar, but involving retinal asymmetry, is the personal difference that affects the reading of chronograph records from the record sheet, that is, the fixing the relative distance of the jag in the line that marks the transit from the other jags that mark the seconds.

Surprise is a psychological factor that is hardly to be expected in transit observations where the observer sees the star advancing in the field and knows precisely what he is to do when it reaches the reticle. It seems to be a fact, however, that records at the first wire are a little less certain than at the rest.[2] The transits of the wires after the first recur at intervals with which an observer in time becomes so familiar that he could even make some sort of a record at the remaining wires for a star that disappeared after crossing the first. The knowledge of the exact time at which the transit is going to happen enables him to make full mental and physical preparation for its observation.

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[1] B. Peirce : Proc. American Acad. of Arts and Sciences, IV (1860), 197.

[2] Argelander : *Vierteljahrss. d. Astron. Gesells.*, 1872, p. 16, referred to by Downing (*Monthly Notices*, XXXVIII), who adds figures for probable error at first and ninth wire in observations of the sun.

Knowledge of the amount and direction of the personal error may lead to change in it. C. Wolf was able by three months' practice to reduce his anticipation in eye and ear observations permanently from 0.30 s to 0.11 s. Rogers found the assumption that he observed too late sufficient to quicken his observations by more than 0.03 s. Wolf probably had, like Rogers, the expectation of realizing such a change under such circumstances, and that expectation may have co-operated in producing the result.

The foregoing exposition of circumstances under which the personal equation is variable probably does not exhaust the catalogue. Everything, physical or psychical, that influences the nervous mechanism influences in some degree the time of perceiving and recording the perception.[1] Psychological experimenters have found measurable differences in reaction times depending on attention, practice, fatigue, intensity of stimulus, effect of drugs, emotional states and disease, and other differences that some, at least, have thought due to grade of education, race, age, sex, quality of stimulus, and temperature.

I may sum up the general result of the preceding pages as follows: The personal equation by both methods varies from assignable and unassignable causes. Among the former are the nature of the object (disk or star), its magnitude, its direction and rate of motion, and sundry instrumental and psychic conditions. The explanations of the changes, more or less conjecturally advanced, point to individual differences in the effect of irradiation, in the structure or action of the eye, in habits of anticipation or delay in observing, in the form of recording (Wundt), and in the direction of attention. It is impossible to establish by the observations and experiments described the certain causes of the differences found. The investigations of the astronomers have generally had the very practical aim of discovering the corrections to be applied to their observations. They have therefore tried to reproduce as nearly as possible the complicated conditions of their regular observations. It is for this reason that such a review as has been made is much more fruitful in problems for physiology and psychology than in generalizations. For the purposes of these two sciences the experimental conditions must be simplified and varied, not complicated and held to a typical form.

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[1] How responsive that mechanism is appears in such experiments as those of Lombard on the Knee-jerk, AMERICAN JOURNAL OF PSYCHOLOGY, Vol. I, p. 5.

On the general question of the nature of personal equation itself apart from its variations, the experiments have been a little more explicit and the work of the astronomers and psychologists has come nearer together. It is hoped to treat of the nature and cause of personal equation in the concluding article of this series.

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

On the Relations between the Function, Structure, Origin, and Distribution of the Nerve Fibers which compose the Spinal and Cranial Nerves, being the Marshall Hall prize oration. WALTER HOLBROOK GASKELL. Read May 24, 1888. Vol. LXXI of the Medico-Chirurgical Transactions of the Roy. Med. and Chirurg. Soc. of London.

In this "Marshall Hall prize" oration, Gaskell has reviewed his work on the nervous system since 1881. Taking this account of the author as a guide, it will be worth while to run over his important contributions, despite the fact that some of them are already several years old.

In 1881 Gaskell was able to show, by the use of a new method, that the vagus nerve not only inhibited the rate and force of the heart's beat in the frog, but was also able to accelerate the rate and augment the strength of the contractions (Proc. Roy. Soc. XXXIII, 1881–2). A special feature of the method used was a clamp by which any desired pressure could be applied between two portions of the heart, and thus an artificial resistance to the passage of an impulse from one part to the other introduced. This led to the discovery that the vagus nerve in the frog was in reality a vago-sympathetic, the vagus fibers causing inhibition, and the sympathetic acceleration and augmentation; that the two sets of fibers were present both in the tortoise and the crocodile, and that in all cases the accelerator fibers reached the heart by the same course as in the mammal (Journ. Physiol. Vol. V, 1884–5.)

Further, Gaskell was led to the view that the nature of the action of these cardiac nerves was directly on the muscle, and therefore inhibition is something taking place in the heart muscle itself, and is not to be explained by the interference of nerve action outside the muscle (Cong. périod. internat. d. sc. méd. Compt. rend. 1884, Copenh. 1886).

If the after-effects of stimulation are considered, it becomes plain that the result of vagus stimulation is to put the heart muscle in a state of repair in which it is capable of doing more work; while the accelerator, by exhausting it, brings about an opposite condition. Regarding, then, principally the constructive action of the vagus, and the destructive action of the accelerator, they are designated respectively anabolic and katabolic nerves (Journ. of Physiol. Vol. VII, No. 1). This is the most extensive and perhaps the most important of these papers.

The notion of a double nerve supply, similar to that in the heart, to the different tissues of the body, was supported by the study of the muscular tissue of the rest of the vascular system, the muscular tissue of the alimentary tract and of glands, and has led to the suggestion that probably all the tissues of the body are supplied with anabolic and katabolic nerves.

The question as to whether tissues are supplied in this way is but another form of the question whether trophic nerves exist, using the term in its most general sense; and it is well known how much attention has been given to that question of late. The fact, too, is not without significance that those who are working with Gaskell, or under his direction, are at present studying this very point of the double nerve supply of the various tissues.

While these physiological ideas were being worked out, Gaskell made an anatomical observation that has been of fundamental importance to his work. On examining the sympathetic cardiac fibers going to the heart of the tortoise, he was much struck by the difference between them and the vagus fibers; the sympathetic fibers being unmedullated between the heart and the ganglion from which they emerge, while the vagus fibers are medullated throughout their course. Here was a distinct morphological difference between the anabolic and katabolic nerves, and further investigation showed that it held good for all animals examined. In studying these two sets of nerves, and following them back to their origin in the central system, he developed the large number of new facts and new points of view which are contained in the paper last cited. It was known that the accelerator nerves for the heart passed out in the anterior spinal nerve roots. On examining the upper dorsal and lower cervical roots in the dog, they were found to be free from non-medullated fibers. At the same time it was noticed that the anterior roots of the dorsal region differed from those of the cervical, in containing a group of very fine medullated nerve fibers. These very fine fibers were found to form the bulk of the white Ramus communicans by which the sympathetic ganglion was in connection with the central system.

The course of the accelerator fibers was then clearly in the anterior roots, where they were represented by a group of excessively small medullated fibers, thence to the appropriate ganglion, where the medullary fibers ceased and were continued on as non-medullated fibers. Further study showed that in the ganglia it was the ganglion cells that gave rise to the non-medullated fibers, and that the small medullated ones reaching the ganglion from the cord were in connection with these cells, the exact mode of connection not being worked out. When it is said, then, that these fibers lose their medullary sheath on passing through the ganglion, it must be remembered that, strictly speaking, the medullated fiber probably terminates in the ganglion cells in one way, while its physiological equivalent arises from these cells in quite another way. The anatomical difference between anabolic and katabolic nerves, then, depended on the point where the latter lost their medullary sheaths. This connection between the white Ramus communicans and the structure of the anterior root led to an examination of all the spinal roots in the dog, with the result of showing that only where these bundles of small medullated fibers were present are there any white Rami communicantes or homologous structures—in all other cases the Rami communicantes are non-medullated. It would necessitate a lengthy digression to give the facts presented in connection with the visceral ganglia, and they are here neglected, as it is of more importance to get at Gaskell's idea of what constitutes a complete segmental spinal nerve, and see the relations of his view to the interpretation of cranial nerves with which he concludes. This much may, however, be said regarding the small medullated fibers: 1. A cervico-cranial outflow of them occurs in connection with the vagus and upper roots of the spinal accessory nerves. 2. A thoracic outflow occurs between the second dorsal and second lumbar roots; and 3. A sacral outflow occurs in the roots of the second and third sacral nerves.

This marked inequality in the outflow leads Gaskell to a number of other considerations. In the first place, the gaps between these outflows are filled up by the plexuses for the anterior and posterior extremities—a fact which receives no special interpretation. The limits of the outflows correspond so closely to the limits of Clarke's column of cells that in Gaskell's opinion they force one to the conclusion that this group of cells is connected with the visceral nerves. The very roundabout course of the constrictor nerves of some of the blood-vessels and of the sympathetic glandular nerves has here an explanation, for they all come apparently from the thoracic outflow, and, losing their medullary sheaths in the ganglia of the main sympathetic chain, pass in various directions to their destinations. "Further," he adds, "I was able to point out how this appearance and distribution of small medullated fibers corresponded to known facts as to the distribution of the nerves governing the muscles of the alimentary tract; how the known cases of vaso-dilator nerves and the secretory nerves of the glands were all characterized by the fineness of the caliber of their fibers and the presence of ganglion cells on their course; that in fact a large class of nerves existed of various functions which might all be called visceral or splanchnic nerves, all characterized by the fineness of their fibers, and by the peculiarity that, although *efferent* in function, they were in connection with ganglia in some part of their course."

Looked at from this standpoint, the sympathetic (so called) and homologous ganglia appeared, not as the representatives of an independent nervous system, but as ganglia occurring in the course of the spinal nerves, and belonging to that system as much as the posterior root ganglia belong to it. The study of these ganglia on the efferent fibers shows them to differ from those on the afferent, in the fundamental fact that the fibers which emerge from them are non-medullated, while those which emerge from the latter are medullated. A means of distinguishing between the two sorts of ganglia is thus given, and the distinctions are utilized when Gaskell discusses the ganglia on the cranial nerves.

Formulating these facts, Gaskell describes a spinal nerve as consisting of an anterior and posterior root, both of which are ganglionated, with this difference, that the ganglion on the posterior root is comparatively stationary, occurring near its origin, while the ganglion on the anterior root is vagrant, occurring as a rule at some distance from the central system. Moreover, these motor vagrant ganglia are in connection with a special group of fine medullated fibers.

Further, there are in all probability sensory nerves which supply the region over which the ganglionated efferent nerves are distributed, and these might be considered as forming a special group in the posterior roots. Still another subdivision was introduced by the consideration of the relation which these fine ganglionated visceral fibers bore to the groups of large motor fibers controlling respiration, deglutition and expression, which are found specially in the spinal accessory and the vagus group. These large fibers thus associated with the small ones corresponded closely with Bell's group of respiratory nerves. Gaskell gets his explanation for this relation of things from certain researches by van Wijhe on the mesoderm segments in the head of selachians.

In this case there is a double segmentation into a dorsal and ventral series of muscle plates, the dorsal series being supplied by the third, fourth, sixth, and twelfth nerves, and a ventral series by the fifth, seventh, ninth, and tenth nerves. The muscles developed from this latter series were those of mastication, respiration, and deglutition, thus belonging to the respiratory group. These coarse fibers that are found associated with the fine ganglionated efferent fibers seem, then, to form a special group. Designating fibers as somatic or splanchnic according as they supply the dorsal or the ventral mesoderm segment of van Wijhe, a spinal nerve may be considered as composed of: 1. A posterior root with a stationary ganglion into which both somatic and splanchnic afferent fibers pass; 2. An anterior root composed of (*a*) a large fibered somatic and (*b*) a large fibered splanchnic portion, both of them non-ganglionated, and of (*c*) a small fibered ganglionated splanchnic portion, the ganglion of which is vagrant.

There are therefore three groups of fibers recognized in the anterior root and two in the posterior, the idea being best supported by the arrangement of the fibers in the upper cervical and lower cranial region.

The groups of cells in the cord are harmonized with this division of the nerves in the following manner. Dividing the cells into somatic and splanchnic, it is found that the somatic efferent and the somatic afferent arise respectively from the ends of the anterior and posterior cornua. The splanchnic afferent are supposed to have their center in the solitary cells of Schwalbe in the neck of the posterior horn. The non-ganglionated splanchnic efferent fibers seem to be connected with the cells of the intermedio-lateral tract situated in the lateral horn, while the small ganglionated efferent fibers are made to arise from the cells of the column of Clarke.

As the discussion of these centers of origin is, however, not fundamental to what is to be said on the homologies of the cranial nerves, it may be left at this point. In the study of the cranial nerves there are a few general points that serve as guides. A complete segmental nerve should exhibit the five groups of fibers just enumerated. It must be determined whether the ganglia on them are stationary or vagrant. It must also be borne in mind that the ventral roots do not need to run free of the stationary ganglion, but may even run through it, as, for example, is the case with the first two cervical nerves of the dog.

Gaskell does not consider his work on the cranial nerves as yet complete, but is willing, nevertheless, to put forward some general conclusions. In the first place, the olfactory, optic, and auditory nerves are left entirely out of consideration. The remaining nine nerves seem to fall into two groups: 1. A foremost group, which in man is almost entirely efferent, namely, the oculomotor, trochlearis, motor part of the trigeminal, abducens and facial; 2. A hindermost group of nerves of mixed character: sensory part of the fifth, glosso-pharyngeal, vagus, spinal accessory, and hypoglossal.

The nerves of the first group resemble the anterior roots of spinal nerves, in that they contain the three groups of fibers considered as typical. Gaskell is inclined to count them, however, only as four nerves, the abducens being regarded as the somatic portion of the motor part of the trigeminal. This foremost group of nerves also resembles a typical spinal nerve in its posterior root, in that they have a ganglion near the place of exit of the nerve from the central system. This ganglion, and the fibers connected with it, are in all cases phylogenetically degenerated, and a characteristic arrangement of connective tissue is all that is left to show where the nerve structures have formerly been. For the third, fourth, and seventh this condition is shown by figures of sections in an accompanying plate. This group resolves itself, therefore, into four segmental nerves, the afferent parts of which have undergone degeneration. In the second group above named there has been no loss of any part, but there is at the same time considerable dislocation of the various components, so that it requires some rearrangement to recognize the five segmental nerves which Gaskell considers to exist here. His statement of the case is this:

"1. The ascending root of the trigeminal, together with the auricular branch of the vagus, contains the somatic afferent nerves. The ganglion for these nerves is mainly the Gasserian ganglion.

2. The ascending root of the glosso-pharyngeal and vagus nerves contains the splanchnic afferent fibers, with the ganglion jugulare for its stationary root ganglion.

3. The hypoglossal, and probably part of the spinal accessory, form the somatic efferent portion.

4. The large motor fibers of the glosso-pharyngeal (?), vagus, and spinal accessory in part form the splanchnic non-ganglionated efferent fibers.

5. The small fibers of the glosso-pharyngeal, vagus, and spinal accessory, with the ganglion petrosum glosso-pharyngei and the ganglion trunci vagi for their motor ganglia, form the splanchnic ganglionated efferent fibers."

From these facts it is plain that the cranial nerves are built upon the plan of the spinal nerves, but are divisible into two groups, the meaning of which is yet to be made out.

Thus far what has been presented has been almost directly quoted from this last article of Gaskell. In calling attention, now, to those points in the argument for which the evidence appears insufficient, or is contradictory to apparently well established results, it is proper to speak only in the most general terms. There is certainly a gap that needs filling, between what has been determined for the anabolic and katabolic nerves of the heart and some other organs and the generalization that all the tissues of the body have a similar double supply. The voluntary muscles at once present themselves as a problem in this connection, and thus far two sets of nerves have not been shown for them. It is, perhaps, suggestive that all those cases in which the double supply is known to exist are in organs not under the control of the will. The association of the small white fibers in the anterior nerve roots with the cells of the column of Clarke, on the basis of the coincident extent of the two structures, is a view so very contrary to that now held concerning the connections of Clarke's column, that it must be supported by stronger evidence than that presented before it can hope for acceptance.

There is, further, no example of the typical spinal nerve as it has been described. The typical nerve is a composite made up from the consideration of nerves at several levels, and finds its best exemplification in the cranial region which is apparently most modified. The central termination of the other two splanchnic groups has but very little positive evidence in favor of it thus far. In explaining the homologies of the cranial nerves, it is without doubt the nerves of special sense that are the most difficult to bring into line; but it certainly is an open question whether any arrangement of the nerves which leaves them out of account, as that of Gaskell does, is to be looked on as more than tentative. Supposing, however, that these criticisms should be supported by the results of future experiments, there would still remain a most important mass of information which has been the direct outcome of Gaskell's work. In the first place, the idea of anabolic and katabolic nerves, whatever the extent of its application, has a very considerable explaining power. To the neurologists, however, the fact that the so-called sympathetic nervous system can be shown to be an integral part of the spinal system, and not something independent, is of the utmost consequence; for not only is it a distinct anatomical advance, but, like all well grounded anatomical ideas, it disposes of much antiquated physiology of the region. The light which these ideas throw on the cranial nerves has certainly made it possible to attempt some order among this difficult group, and whether Gaskell is exactly on the right track or not, it seems clear that their illuminating power has not been exhausted.

Anleitung beim Studium des Baues der nervösen Centralorgane im gesunden und kranken Zustande. Dr. HEINRICH OBERSTEINER. Leipzig und Wien: Toeplitz & Deuticke, 1888.

In discussing a book that thus purports to be an introduction to the study of the central nervous system, the present state of the literature must be kept in mind. With the development of histological technique, the finer anatomy of the brain and cord began to be studied, and accordingly the last thirty years embrace most of the papers on minute structure. Of course this branch has been subject to the same laws that apply to all histological work, and the rapid increase of contributions in the later years of this period has been enough to appall any author contemplating a book that should represent the present state of knowledge on the subject. The disconnected character of the majority of the contributions which state the anatomical conclusions of the author and leave to some future compiler to knit the results together with those of others, is but the consequence of the comparative absence of general notions under which these special observations can be classified, and of course makes compilation most difficult. Various authors have attacked the problem in different ways. Schwalbe has produced something like an encyclopaedia of the subject. Henle, Meynert, and Wernicke have given rather individual accounts, less controlled by the views of others than is desirable in a manual. The excellent "Zehn Vorlesungen" of Edinger lacked sufficient detail for use in the laboratory, and, it should be added, was not intended for that purpose. There was room therefore, and need as well, for a book that should give a general view and yet contain sufficient detailed description to be used in the laboratory beside the microscope. No doubt Obersteiner is not the first man who has felt the desirability of such a work, but, to most men, the combination of the continuous narrative style which was demanded in such a book, with a full treatment of the evidence, seemed quite incompatible. In meeting this difficulty, Obersteiner has followed the plan of taking what seemed to him the best view, and presenting it, as a rule, with little comment, only now and then stating the more important deviations from it. The plan is simple enough, but to carry it out to the satisfaction of the reader and the illumination of the subject implies a delicacy of anatomical instinct that is, to say the least, rare.

The book is divided into seven sections. The first discusses the methods of investigation under the five subheads of fibering, serial sections, pathological changes and arrested development, the method of comparative anatomy, and the experimental-physiological method. The value of each is briefly stated. When speaking of the method of serial sections he describes the histological technique so far as it applies to the nervous system, and gives a very compact statement of the methods of staining, etc. There is a blemish in the presentation of some formulae, however, that should not occur in a book of so excellent a character, that is, the "cook book" recipes for making various solutions. For example, one is instructed (p. 11) to take "as much haematoxylin as will go on the point of a knife." The thing is too trivial for further comment, but certainly important enough to deserve correction. In explanation of his division of methods, it may be said that by the experimental-physiological he means to describe such a method as was used by Ferrier and Yeo in studying the distribution of the spinal nerves in the monkey, and not the methods of degeneration which are sometimes understood by that term. Further, the methods of v. Gudden, Waller, and Flechsig are all summed under the general head of pathological changes and arrested development.

In the second section, after a couple of pages on the vesicles of the brain, is a description of the gross anatomy of the brain, most admirably illustrated, and followed by an account of the convolutions, in which Ecker is mainly followed. In reading this section the attention is at once fixed by the lucid style of the author, which gives to the descriptions a simplicity and clearness that is not always a distinguishing feature of books on this subject. The impression is unavoidable that the man is writing with the specimen before him. This charming clearness comes out even more strikingly later on, but it may be sufficient to mention it here, adding, though not without fear of a smile from the incredulous, that, by virtue of the form in which it is here presented, the anatomy of the nervous system becomes positively entertaining. It has previously been maintained in these columns that the proper understanding of the structure of the adult brain could not be gained without a clear grasp of its embryology. The absence of a sufficient discussion of the development of the brain seems to us the main and the sole important defect in the book. Perhaps the fact that one may read the book without feeling the lack of the information here demanded is in itself an answer to the above criticism, but we cannot suppress the feeling that a discussion of the embryology would have been of much value to those who will use the book. The descriptions of the plates are full and clear, but there is no order in the arrangement of the descriptive terms, and one is often quite discouraged in searching for a name. Wilder seems to have reached the best solution of this problem, which is really a serious one for all parties concerned: he has arranged the terms alphabetically. It would certainly save time and vexation if some such plan could be adopted here.

In the third section the histological elements are taken up, the description of the nervous elements being followed by that of the non-nervous. Obersteiner here pursues the very instructive plan of describing at the end of each subdivision the pathological changes to which the element is liable. Nothing could be more to the advantage of the student, for not only is pathological material the most abundantly used in the laboratory, but it is as a rule the most instructive, and these few hints as to the significance of the changes are highly useful. The plan of giving the pathological variations is followed in the later sections, and is rather a distinctive feature of the book. In treating of the nervous elements he fails to use the idea that the nerve fiber is an outgrowth of the nerve cell, and that therefore fiber and cell form a unit, but takes rather the traditional course of considering them separately, without indicating such a connection. This is the more remarkable, since not only are the views expressed based, as a rule, on the very latest results, but all the way through there is an evident desire to give prominence to such ideas as have a generalizing value. It is in this section that the results obtained by Golgi come in for discussion. He follows that author on the relations of the protoplasmic processes of the cells and the lack of direct anastomosis between them, but seems almost ultra-conservative on the question of the axis cylinder as brought out by Golgi's method.

The finer structure of the spinal cord, which is the subject of the fourth section, contains the unorthodox argument for the origin of the fibers of the posterior spinal roots directly from cells and not indirectly from the fiber network, as is usually taught. It seems probable that if the facts are regarded in a certain way, Obersteiner's view should be added to the older one, but it can hardly be granted that the new view displaces the old one, as the paragraph indicates.

The next forty pages, forming the fifth section of the book, will be without doubt the most useful portion of the book. It contains the description of some twenty excellent woodcuts based on carmine preparations. The first cut represents a level of the cord just at the commencement of the crossing of the pyramids, and the last is through the middle of the optic thalami, the intermediate sections being taken at the most instructive levels. The location of each section is designated by lines transverse to the representation of the medulla and the basal ganglia. In the cuts themselves one half is made schematic and the other a copy of the actual preparation; a combination which has much in its favor. It is in this section especially that the freshness of the descriptions offers such a pleasant contrast to the portion of most treatises with which it can be compared.

As might be expected, the sixth section is the most extensive, since it comprises a discussion of the tracts in the cord, the cranial nerves, the forebrain, etc. It is in discussing the cranial nerves that there is the greatest opportunity for the display of an anatomical instinct which may enable one to select from the mass of contradictory results, all of which seem about equally well supported, those which are really valuable. The narrative here is crisp and clear, and the confusion brought about by a multitude of names for the same thing is quite dissipated by the simple device of describing the thing itself and leaving the names quite at one side. There are probably several people in the world who will feel greatly indebted to Obersteiner for his description of the lemniscus and of the centers of the cranial nerves. This is, perhaps, the place where the kind of evidence which the author is willing to accept is most clearly recognizable. We do not know that Obersteiner was a pupil of Meynert, but certain it is that he has not that feeling of skepticism towards conclusions based on observation of the normal tissue which is felt by the followers of von Gudden, for example. He is conservative always, but at the same time is willing to interpret much that will have to be demonstrated at some later day. The book terminates with the seventh section, on the envelopes of the brain, followed by a good index.

By what has just been said concerning the kind of evidence that appeals to our author, we would not be understood to impute one-sidedness, save so far as every man has some bias in that he does not exactly agree with his neighbor. It is eminently a spirit of fairness that characterizes the book, and it is quite free from the narrowness of a special school. No one method or point of view can give a satisfactory survey of the entire field, and Obersteiner is far beyond the not uncommon attitude of mind of those who mingle patriotism with science, and look upon the method discovered in their town as the only correct one, or at least the most correct. The book, then, is a laboratory manual of unusual excellence, and, at the moment, is the only one of its kind.

Pathologie und Therapie der Nervenkrankheiten für Aerzte und Studirinde. Dr. Ludwig Hirt. Erste Hälfte, S. 256. Mit zahlreichen Holzschnitten. Wien und Leipzig: Urban und Schwarzenberg, 1888.

This first half of the book deals with the brain, and is to be followed by a second half, of the same size and character, on the spinal cord. Some description of the first half will show the character of the book, for the portion which is to follow is to have the same general construction. The author has made his compilation compact, and avoids the discussion of the more unsettled questions. The arrangement is highly systematic. There are three sections, dealing respectively with the diseases of the brain envelopes, the cranial nerves, and the brain substance; each cranial nerve, for example, is discussed in a separate chapter. The chapter is opened by a brief anatomical description, followed by the diseases, diagnosis and treatment, and terminated with a very fair collection of references to the literature. The book is well illustrated throughout, by cuts from standard works and a few that are original.

On Some Results obtained by the Atrophy Method. E. C. Spitzka and R. Mollenhauer. Journ. of Nervous and Mental Disease, N. S. Vol. XIII, No. 6, June, 1888.

In a kitten two days old the left crus cerebri was severed by means of a cataract needle, which was inserted through the skull and pressed downwards and outwards at a point in front of the anterior pair of the corpora quadrigemina. The animal was killed just ninety days later. At the time of operation it was two days old and the eyes were closed. It first showed strong circus movements to the right and had always to be fed. Its development throughout was retarded, and although at first it was playful, it later became rather sluggish. When the brain was examined it showed the left hemisphere, thalamus, oculomotor nidus, and associated parts completely atrophied. The other hemisphere was, as usual in such cases, more than the normal size and extended over the middle line. The optic tract, the oculomotor root and the pyramid on the left side had all atrophied, while remaining normal on the right side. The corresponding corpus quadrigeminum and the optic nerve had diminished in size in accordance with these atrophies. On sections through the region of the pons, the mesial and intermediate portions of the lemniscus, the posterior longitudinal fasciculus, and a crossed bundle of large fibers near the raphe have atrophied to various degrees. Passing caudad, the atrophies become less and less marked. When compared with certain results obtained by Forel and v. Monakow, the *plus* of atrophy may be attributed to the destruction of the thalamus, and thus the course of certain tegmental fiber systems is established.

Comparison of the Convolutions of the Seals and Walrus with those of the Carnivora, and of Apes and Man. Sir WILLIAM TURNER. Jour. of Anat. and Phys. Vol. XXII, 1888, pp. 554–581.

Sir William Turner, in his report on the seals collected during the voyage of the Challenger (*Zool. Chall. Exp.* Part LXVIII, 1888), describes the brain of the elephant seal (*Macrorhinus leoninus*) and of the walrus (*Trichechus rosmarus*). In connection with this description, he compares the cortical areas of the cerebrum in these animals with those found in the Carnivora proper, and in apes and man. After referring to the accounts given by Lauret, Broca, Owen, and Krueg, he describes the fissures and convolutions of *Phoca vitulina*. On the outer surface of the hemisphere in this mammal is a distinct fissure of Sylvius, with its Sylvian convolution, the anterior limb of which is narrower than the posterior, and at its commencement concealed within the fissure of Sylvius. In the walrus, and also in the eared seal, bear, otter, coati, badger, and ratel, this narrowing and depression of this limb of the Sylvian convolution exists. The convolutions and sulci of *Macrorhinus* correspond in essential points with those of *Phoca*. From an examination of the brains of *Trichechus*, *Phoca*, and *Macrorhinus*, Turner is inclined to consider these animals as approximating, in the arrangement of the convolutions of the outer face of the hemisphere, to those carnivora which possess four tiers of convolutions in relation to the fissure of Sylvius, this arrangement being present in the dog, jackal, fox, and wolf. He found that the area named by Mivart the ursine lozenge was rudimentary or not definitely defined in the seals and walrus. Turner next compared the convolutions on the mesial and tentorial surfaces of the hemisphere in the Pinnipedia, with the corresponding ones in the brains of several of the Canidae and Felidae. The crucial fissure, he found, varied materially in its position in the Carnivora and Pinnipedia. In the seals and walrus it was so far forward as not to be seen on the dorsum of the hemispheres, but only at the anterior end of the cerebrum. In the cat and tiger it was visible in about the anterior fourth of the dorsum of the hemispheres; in the dog, weasel, ferret, and coati, at the juncture of the middle and anterior third; in the badger, polar bear, and ratel it was further back, just in front of a line dividing the dorsum of the hemispheres into an anterior and posterior half. In comparing the cerebrum of the Carnivora and Pinnipedia with that of man and apes, Turner finds a morphological correspondence between certain of the convolutions and fissures. In the walrus and seals the Island of Reil, he says, may find its representative in the anterior limb of the Sylvian convolution, which is more or less hidden within the fissure of Sylvius. If this indication be true, he believes that the Island of Reil, which in the brain of the ape, and more so in man, is entirely concealed within the Sylvian fissure, is either the homologue of the Sylvian convolution of the carnivorous brain, or that the Sylvian convolution in the Carnivora potentially represents both that convolution and a rudimentary insula. He thinks there can be no doubt that the anterior and upper part of the splenial fissure in the brain of the Carnivora and Pinnipedia corresponds with the fissure which is known as calloso-marginal in man and apes. In several of the Canidae the splenial fissure was continuous with the crucial fissure, but in the cat, tiger, coati, and polar bear they were not continuous; whilst in *Phoca vitulina* the two fissures were continuous in one hemisphere, but not in the other. From the anatomical data and experimental evidence Turner thinks it may be assumed that the fissure of Rolando is homologous with the coronal fissure in the carnivorous brain. He says in conclusion: "From the point of view of the hypothesis of evolution there would be no reason to think that the smooth-brained lower apes had originated out of the Carnivora, at least after the cortex of the cerebrum in this latter order had begun to assume a convoluted arrangement. If they had been derived from a carnivorous animal with a convoluted brain, then in all likelihood the convoluted character of the cerebrum would not have disappeared in the process of evolution. If the higher apes have been derived by descent from the lower apes, then the hemispheres in the former, with their complex arrangement of fissures and convolutions, have been evolved from a smooth-brained stock, and not from an animal with such an elaborate arrangement of convolutions as is possessed by either a dog or a seal. Hence, the acceptance of this hypothesis is not inconsistent with the fact that the convolutions of the brain in the apes assume from the first their own methods of arrangement, and not necessarily that of the orders of mammals with convoluted brains which are lower in the series. Beyond, therefore, a certain general correspondence in the arrangement of those fundamental parts of the cortex which serve a similar purpose in these various orders, one does not find it possible to determine the presence of convolutions arranged in a precisely corresponding manner in the brains of the Carnivora and Pinnipedia on the one hand, and of man and apes on the other." F. T.

The Morphology of the Vagus Nerve. THOMAS W. SHORE. Jour. of Anat. and Phys. Vol. XXII, 1888, pp. 372–390.

After giving the anatomy and development of the vagus nerve in Petromyzon, Elasmobranchs, the frog, Amniota, and the chick, and after some discussion of the morphological value of its various portions in these types, the author concludes that there is evidence to show, first, "that the vagus is a 'compound nerve,' but not in the sense generally supposed; it is rather a compound of the visceral rami of the anterior spinal nerves, and of the remnants of the brain-ganglia and lateral cords of the nervous system of invertebrata, than of several metameric nerves." Secondly, "that the visceral part of the vagus of fishes includes the branchial nerves, and has arisen from a coalescence of the visceral rami of the anterior spinal nerve segments, the corresponding motor and sensory somatic branches of which have remained separate." Thirdly, "that the ganglia of the cranial nerves (5th, 7th, 9th and 10th) are the representatives of the brain lobes of Nemertea, and probably of the cerebral ganglia of Annelida and Arthropoda." Fourthly, "that the ramus lateralis is of extreme ancestral origin, and is equivalent to the lateral strands in the nerve plexus of Nemertea, to the main nervous system of Annelida and Arthropoda, and possibly also to the nerve ring of Coelenterata." Fifthly, "that a study of the vagus nerve throws light on the question of the chordate ancestor, and does not tend to support the views of Dohrn and his school." Sixthly, "that the value of the vagus in deciding the question of the segmentation of the vertebrate head has been much overrated."

F. T.

A Rudimentary Sense Organ.

(1). *Zur Deutung und Entwickelung des Gehirns der Knochenfische.* Rabl-Rückhard. Archiv. f. Anat. u. Physiol. (Anat. Abth.) 1882.

(2). *Untersuchung über das Gehirn der Petromyzonten.* F. Ahlborn. Zeit. f. wiss. Zool. Bd. XXXIX, 1883.

(3). *Ueber die Bedeutung der Zirbeldrüse.* F. Ahlborn. Ibid. Bd. XL, 1884.

(4). *Zur Anatomie u. Entwickelung der Epiphyses bei Amphibien und Reptilien.* H. W. De Graaf. Zool. Anz., March, 1886.

(5). *On the Presence and Structure of the Pineal Eye in Lacertilia.* W. B. Spencer. Quart. Jour. Micr. Sci., October, 1886.

(6). *The Parietal Eye of the Cyclostome Fishes.* J. Beard. Ibid. July, 1888.

(7). *The Pineal Eye in Extinct Vertebrates.* E. D. Cope. Am. Naturalist, October, 1888.

(8). *Recherches sur le développement de l'epiphyse.* P. Franchotte. Archiv de Biologie, Tome VIII, 1888.

Amongst human anatomists there has existed much doubt and consequent diversity of opinion concerning the function of the pineal gland or epiphysis of the brain.

Recently—within the last three years—we have advanced far towards a solution of the problem, by a comparative study of this so-called gland in the different classes of vertebrates.

In 1882 Rabl-Rückhard (1) was the first to throw some light upon the function of the gland. He suggested, from his study of its development in the trout, that the pineal body might represent an eye and was comparable to the paired lateral eyes, but failed to give properly the anatomical structure of this median eye.

Ahlborn (2 and 3) carefully described its structure in the fish Petromyzon in 1883; and in 1884, discussing the nature of the pineal gland, says he comes to the conclusion that it may be regarded as the rudiment of an unpaired eye, and to be compared perhaps to the median eye of the lancelet and of the tunicates.

In 1886 de Graaf (4) first conclusively showed that in one of the lizards (Anguis) the pineal gland is modified into a structure directly comparable to an invertebrate eye. He further showed that in amphibia and reptiles this body arose as a hollow outgrowth of the hind part of the first brain vesicle—thalamencephalon—that this outgrowth became constricted off distally into a hollow sphere, and the proximal portion formed a stalk connecting the sphere or bulb to the brain.

The general attention of naturalists was drawn to the subject by an admirable article of W. Baldwin Spencer in the Quarterly Journal for 1886 (5). He confined his research to an exhaustive study of the eye in the group Lacertilia, and within the same group found all stages of development; in some forms a simple outgrowth—evagination—from the upper wall of the brain, and in others developed into a well defined eye. As to the general form of the epiphysis he says, "The simplest form is seen in Platydactylus, where it has merely the structure of a hollow outgrowth running at right angles to the surface of the thalamencephalon until it reaches the dura mater lining the cranial cavity. In Hatteria, on the other hand, we have a form in which specialization is carried to its farthest extent, with the result that the epiphysis becomes modified into three parts, (1) a proximal part, still hollow, and connected with the brain roof, (2) a median solid pineal stalk serving to connect the former with (3), the distal portion differentiated into an optic cup. These forms may be taken as two extremes, the gap between which is filled up by various modifications."

In his examination of Hatteria he made out the following structures. An absence of pigment in the skin indicates the position of the median eye, and beneath it lies that organ in a foramen between the parietal bones. Beneath the unpigmented skin there is found a layer of connective tissue, which would prevent an image being formed in the eye, and indicates in this particular a considerable amount of degeneration, even in this highly organized structure. He compares the general shape of the eye to a cone with its base towards the surface and the pineal stalk or nerve connected with the apex. The walls of this vesicle are divided into (1) an anterior portion (base of the cone) which forms a lens, and (2) a posterior (sides of cone) which is the sensitive area or retina.

A study of this retina has yielded most interesting results and we cannot do better than quote the author's own words. He says: "The retinal elements are arranged in a manner typical of invertebrates, *i. e.* the rods lie on the inner side bounding the cavity of the optic vesicle, the nerve entering posteriorly and not spreading out in front of the rods. In the same vertebrate animal we thus find eyes developed on both vertebrate and invertebrate types, both being also formed from the modification of the walls of hollow outgrowths of the brain," and, it may be added, both arising from the walls of the embryonic fore-brain, approximating to the same place of origin in the early stages of development. The retina is found to consist of the following structures: "a rod-like layer of bodies enveloped in deep pigment," which rods show a remarkable specialization in the optic axis of the eye. Here they are elongated to twice their ordinary length, and are connected at their proximal ends (outer ends in relation to center of cup) with a group of nucleated cells, which cells are connected with the fibers of the optic or pineal stalk.

The retinal elements to the right and left of the axis are connected at their proximal ends with nucleated bodies, and these in turn send down processes into the pineal stalk. Other interesting structures are described, but for details his figures should be consulted. Longitudinal vertical sections through the head of the lizard show that the eye is connected with the epiphysis of the brain by a solid well-marked stalk—the pineal stalk. The elements of this pineal stalk or nerve have "much the appearance of those found at an early stage in the developing nerve of the paired eyes, that is, they resemble cells which are undergoing a process of elongation so as to form long fibers."

The pineal gland has been found in fishes, amphibians, reptiles, birds and mammals in varying stages of degeneration, reaching its greatest development in reptiles, and is least developed (or most degenerate) in mammals. In some forms, viz. the lizards especially, a foramen is left between the parietal bones. In others even this is obliterated, thus in the amphibian cutting off the bulb from the pineal stalk, and enclosing within the skull in mammals all that remains of the pineal eye.

There is an eye developed in the median line in the Tunicates and in Amphioxus, and since Kowalevsky showed in 1866—and his results have been fully verified—that these two groups of animals must be included in the same great group with the vertebrates, the temptation to homologize this eye with the pineal eye is very strong. And when we further see that in both these groups the median eyes develop from the upper surface of the nerve tube at its anterior end, the resemblance becomes very striking. On the other hand, when we see how easily any epidermal cell becomes specialized to receive light vibrations—so much so that this property is often ascribed as one of the general properties of protoplasm—we must be very cautious before we homologize these structures; especially so when they differ much in details. Mr. Spencer discusses this relation and concludes "that the eye of the larval Tunicates is probably homologous with the pineal eye, and that the eye of Amphioxus may be in no sense homologous."

In the *Quarterly Journal* for July, 1888, Dr. J. Beard (6) has given an account of the structure of the pineal eye as found in the Cyclostome fishes, including the forms Ammocoetes and its adult condition Petromyzon, and Myxine. This very ancient group of fishes was chosen "on account of their exceedingly primitive characters," hoping in this way to get some clue as to the phylogeny of the parietal eye.

He finds the structure strictly comparable to the eye of the higher forms, and in all these forms the pineal bulb is connected by means of a stalk to the brain. Distally the wall of the bulb forms an unpigmented lens, beneath which is a cavity filled with fluid, and the proximal wall forms a retina. The histology of the retina in Petromyzon is one of the most interesting results of his research. With a low power of the microscope it is seen to be made up of three layers: "(1) an inner layer of rods (inner in relation to center of eye), which also contains pigment; following this, (2) a layer of nuclei; and outside this, (3) a granular layer containing a few ganglion cells." The inner layer of rods (1) shows an exceedingly interesting structure, and, to quote his own conclusions, "the end elements are shown to be of two kinds, comparable, it seems to me, to those in the retina of ordinary eyes. By far the most numerous are the long rods; but in addition, and between the latter, one finds a few cones." Both rods and cones contain nuclei, and are directly continuous with the nucleated cells of the nuclear layer (2). These cells are in turn continuous with the ganglion cells of the outermost layer (3), which send processes into the pineal stalk.

Beard's conclusions as to the method of evolution of the pineal eye are exceedingly interesting, but very hypothetical. He says, "I think the development does show that the parietal eye is a slightly later development than the paired eyes, and that the organ has developed in connection with the paired eyes." He starts with the views of Balfour and others, that the paired eyes once opened dorsally "on the surface of the unclosed neural plate," and as the nerve plate gradually sunk below the outer surface of the body, the "neural plate and eyes got shut in," and at the same time the eyes received light laterally, that is, from the sides of the body, or on the ends of their retinal cells which trail out into the nerves. Beard supposes that at the time of closure of the neural tube, a portion of the primary dorsal light-sensitive area became pinched off, as the median eye, which, as we have seen, would still receive its nerve supply from behind (invertebrate type); that by the involution of the optic cup of the lateral eyes they would receive their nerve supply on the same surface which received the light (vertebrate type).

Prof. Cope (7) has examined the skulls of some extinct vertebrates with reference to the pineal or parietal foramen. Referring to the skulls of two very old fishes, he says: "The structure of the primitive vertebrates strongly indicates the origin of lateral or paired eyes from a single median eye, such as is found in Tunicata." "Among North American extinct reptiles," he believes that Diadectes "relied exclusively on the pineal eye for the sense of sight," while in others in which the parietal foramen is closed, casts of the brain show an extemely large epiphysis, and at the same time in Belodon a communication with the orbit (of the lateral eyes) is established.

The work of P. Franchotte (8) on the epiphysis in Anguis covers about the same ground as that of de Graaf and Spencer on that animal.

Thus all the evidence which has come in points conclusively to the fact that the pineal body or gland developed primarily as a median pineal eye. A study of extinct forms shows a larger parietal foramen than found in existing forms, indicating in some, perhaps, a better developed pineal eye.

A study of living forms indicates an absence of function as an organ of sight, for in none are *nerve fibers* discovered connecting pineal eye with brain, though the nerve stalk is there. All the essentials necessary for sight are shown to have *at one time* existed, but the pineal body must now be relegated to the class of rudimentary organs. T. H. MORGAN.

The Pineal Body (Epiphysis Cerebri) in the Brains of the Walrus and Seals. Sir WILLIAM TURNER. Jour. of Anat. and Phys. Vol. XXII, 1888, pp. 300-303.

In this paper, which was read before the Royal Society of Edinburgh, in December 1887, Sir William Turner makes some interesting statements respecting the appearance, position, and relations of the pineal body in the brains of the walrus and seal. In the brain of the former animal this organ is of unusual size, pyriform in shape, with the apex directed forwards to the optic thalami, to which it is attached. The base is free, and projects backwards so as to be visible, when the brain is examined from above, in the mesial longitudinal fissure between the two cerebral hemispheres. In *Phoca vitulina* the pineal body resembles that of the walrus, in possessing three surfaces and having its apex forwards. It projects behind the corpus callosum, and rests on the corpora quadrigemina and the anterior part of the middle lobe of the cerebellum, but does not appear between the two hemispheres of the cerebrum, when the brain is looked at from above, unless the hemispheres are drawn apart. The pineal body in *Macrorhinus leoninus* has the same shape as in *Phoca*, and possesses similar relations to the cerebrum and cerebellum. In the seals the epiphysis cerebri is larger than in mammals generally, and in the walrus it is about twice as large as in the seals. F. T.

II.—HYPNOTISM.

Eine experimentelle Studie auf dem Gebiete des Hypnotismus. Dr. R. v. Krafft-Ebing. Stuttgart, 1888, pp. 80.

Prof. Krafft-Ebing has studied in detail a single case presenting peculiar hypnotic manifestations. The subject is a young woman of neurotic ancestry, whose own career is typically hysterical. Fearing her father's opposition to a love-match with her cousin, she went to a convent, where she seems to have been hypnotized by some of the sisters and urged to steal money. Fearing detection, she escapes, earns a precarious living, assumes male attire and becomes a private tutor for several years. She is often severely ill with hysterical attacks. Symptoms of kleptomania, of sexual perversion, and of suicidal tendency are also evident. At the time of investigation she is hemianaesthesic, the right side being affected, including the sense organs; and is subject to hystero-epileptic attacks. She is easily hypnotized, and often falls into a somnambulic condition which Prof. Krafft-Ebing takes as autohypnotism. She rather objects to being hypnotized, and though her story is not perfectly truthful, simulation is regarded as out of the question. All the usual hypnotic phenomena can be well demonstrated in her case, but only a few of the most striking call for remark. The ordinarily involuntary functions seem especially controllable in her hypnosis. If the shape of a letter, a glass cylinder, or scissors be held against her skin, with the suggestion that the article is red-hot, a fully developed scar is formed in the shape of the object applied. The same can be taken away by a suggestion, and if transferred from one side to the other becomes reversed as in mirror-script. The magnet has a powerful influence over her, inducing contractures, but as this influence is shared by any object in contact with a magnet, and the magnet itself fails when not in the hand of Prof. Krafft-Ebing, suggestion (perhaps by temperature changes) is evidently the *modus operandi*. She easily takes attitudes appropriate to suggested emotions and *vice versa*. But the experiment with drugs at a distance à la Luys is entirely without success. She easily assumes foreign roles, making her actions and handwriting suitable to the character suggested, and carries out fantastic negative hallucinations (*e. g.* that only the head and arms of a man are visible, the appearance causing great consternation), as well as post-hypnotic suggestions, with automatic accuracy. Her time estimates are also remarkably exact; she will sleep the number of hours suggested, and many of her crises have been averted by such suggestions. This study is a very valuable one, and leads the author to the suggestion-hypothesis as the clue to all the phenomena. The case is full of interesting details, and may be taken as the type of hystero-epileptic hypnotism.

Die Rolle der Suggestion bei gewissen Erscheinungen der Hysterie und des Hypnotismus. Kritisches und experimentelles. Dr. ARMAND HÜCKEL. Jena, 1888, pp. 72.

The points brought together in this brochure seem destined to be the ones about which the most interesting and important discussion in hypnotism will center; and to Dr. Hückel will belong the credit of first suggesting, in a convenient and systematic form, this important line of argument. The point at issue is the crucial distinction between the schools of Nancy and of Paris, between those who regard the psychic element of suggestion as the key to all the phenomena, and those who recognize physical influences as of particular and definite significance in the typical hypnotic manifestations. The chief points to be proved against the Charcot school are the production of all their characteristic phenomena by suggestion merely, without physical aid, and the explanation of how suggestion entered into the doings of the Charcot subjects. The former of these points has been frequently established. Any and all of those peculiar phenomena obtained by Charcot by a pressure here, a pass there, have unquestionably been produced by simple suggestion; and, moreover, the fact that the same manipulations have turned out differently everywhere else from what they do in Paris is of itself quite suspicious. Add to this that on new subjects, the same manipulations, if performed without the subject's understanding their object, will fail. The second point, however, is not so easily disposed of. How can we explain the "transfert," the action of the magnet, the effect of metals, the transition from one stage to another, hemi-hypnotism, and so on? All this, says Dr. Hückel, is either the natural guessing of the subjects or the unconscious suggestion of the operators. In several cases, gold alone of all metals brought about the desired "transfert"; the most precious of metals is, by an analogy not difficult to appreciate, regarded as most efficacious, and such a train of thought will probably be the same in nearly all minds; it is a predictable preference. That gold has no specific influence is proven by the fact that gold believed to be copper had no effect, and copper believed to be gold had. So when a magnet is brought out before a subject, the inference is not far off that something peculiar is to happen, and if the operators confine their attention to one arm, something begins to happen to that arm; then when attention is transferred to the other side, the inference is drawn that the result is to be transferred. When once this result is found to please the operators, and other subjects take the hint from this, a clique is unconsciously formed, and by mere contagion the phenomena take a definite and characteristic form. Thus the dozen patients who have demonstrated so much for Charcot have undoubtedly established a certain *esprit de corps* that induces all to go through the same performances. The supreme importance of unconscious suggestion is not to be exaggerated; as soon as this influence is duly recognized, we may expect uniform results, and not before. The case is given of a patient, never before placed under the influence of the magnet, who was asked to come into the room when the physicians were busy, and where, finding but one chair vacant, she naturally seated herself upon it. It had been arranged that this chair was close against a closet in which was a powerful magnet, constantly in action during the *three-quarters of an hour* that she was kept waiting, but without any result. When, however, the magnet was placed at her elbow, even with the current off, a marked effect resulted; proving conclusively the subjective nature of this influence. To show the same with regard to a psychic "transfert," it was arranged that patient A came with patient B to the suite of rooms of operation, where, unknown to A, patient C was placed; A entered one door, was placed with her back to the door leading to the room in which she naturally supposed B had been led, but which in reality was occupied by C, B having been dismissed in the hallway. B suffered from choreic movements, and instructions were given to C (whom A believes to be B) to remain quiet and allow a magnet (not magnetized) to be placed at her elbow; the patient caught the idea, and contractions and movements soon occurred as though the ailment of the absent B had been transferred to A. Here is "transfert" obtained purely by spontaneous suggestion. The same explanation Dr. Hückel uses for all of the apparently physical phenomena of hypnotism, arguing everywhere that the results are expected, are according to an analogy which the subject appreciates, are quite *natural*, and that unconscious suggestion is the most fertile of all sources of error.

Der Hypnotismus und seine Strafrechtliche Bedeutung. Dr. August Forel. Zeitschrift für die gesamte Strafrechtswissenschaft, IX, 1, 2. Berlin and Leipzig, 1888, pp. 65.

Dr. Forel, the renowned alienist, gives a most admirable presentation of the main facts of hypnotism, for the benefit of the legal profession, who, he believes, will soon have to busy themselves with the possible criminal acts involving this condition. Dr. Forel is a warm adherent of the Nancy school, having derived his interest in the phenomena from Dr. Bernheim himself, and having succeeded in obtaining in Switzerland precisely the same results so brilliantly demonstrated at Nancy. While his exposition contains little that is new, it is extremely well arranged, abounds in accurate and helpful distinctions, and emphasizes strongly the affiliation of the hypnotic sleep with ordinary sleep. Suggestion is the secret of all the phenomena; when we go to sleep we do it by assuming an accustomed attitude, in an accustomed place, and so on. All this is an auto-suggestion. Education is largely a matter of suggestion skillfully applied; one teacher excels another in the art of suggestion. Some individuals readily act under the influence of another's advice or will; others are born to command, carrying with them a manner that enforces obedience; witness Napoleon. Even those who are hypnotizable do not altogether lose their individuality, and a criminal suggestion is more easily carried out by persons with a weak moral training. That, however, real dangers exist in this direction, Dr. Forel fully believes, and suggests methods for the detection of such crime. But he admits the problem to be in an unsatisfactory condition, and that when once this method becomes general, the courts will have to act promptly. One of his remedies is to suggest to each patient that none but he (Dr. Forel) can hypnotize them. The main source of danger comes from the amnesia following a hypnotic suggestion; the patient believes that his motives are his own, and thus experimentally proves the dictum of Spinoza, that "the illusion of free will is nothing else than the ignorance of the motives of our acts."

Ueber Hypnotismus. Dr. HERING. Sammlung naturwissenschaftlicher Vorträge, Dr. Ernst Huth, II, 2. Berlin, 1888, pp. 16.

A somewhat rambling general address, describing in an unsystematic and uncritical tone the chief facts of hypnotism. The author represents no particular point of view, and has evidently not gone very deep into the topic. He was urged to give the address by the interest aroused in the phenomena by a traveling mesmerist.

Hypnotismus und Wunder. Ein Vortrag mit Weiterungen. MAX STEIGENBERGER, Domprediger. Augsburg, 1888, pp. 23.

Even the church has entered the arena of hypnotism. A passing analogy between the trance states to be found in hypnotics and the religious ecstasies of saints is sufficient to arouse in the author a fear lest the accredited church miracles will lose their hold upon the people. He therefore feels himself called upon to denounce hypnotism as partially the work of demonic agents, and to show the radical difference between miracles and hypnotic wonders. He argues that as long as hypnotism cannot explain *all* the wonders of church history, it is idle to consider it at all. One could hardly expect a sympathy of attitude or logic between the church and science on this point, but it is curious to see what shape this mutual misunderstanding takes.

L'Hypnotisme expliqué dans sa nature et dans ses actes. Mes entretiens avec S. M. L'Empereur Don Pedro sur le Darwinisme. Dr. CONSTANTIN JAMES. Paris, 1888, pp. 66 and 23.

It is with the first of these papers that we have to do. It contains a very curious collection of remarks upon hypnotism; a word or two on the nervous system; a clipping from the newspaper describing one of Dr. Luys's hypnotic seances, and so on. The author does not believe in hypnotism, and believes it creates diseases rather than cures them. It is dangerous to public health, it is immoral, and it is not a science at all. There is a good deal of deception about it, and the operators are the dupes. Hysteric subjects are not to be credited, and the whole movement is a "craze" that will have its day and be gone. Dr. James is a peculiar as well as a vague writer, and it is difficult to understand his point of view. Whatever is justifiable in his position seems to be grounded upon an opposition to the sensational and miraculous treatment of hypnotism now so prevalent in Paris.

Hypnotismus und Willensfreiheit. Vortrag gehalten in der Aula der Universität Basel von F. MIESCHER, Professor der Physiologie. 1888, pp. 36.

Like many such addresses, the object here is simply to acquaint the audience with the general features of hypnotism. This Dr. Miescher does by giving the history of the topic, accenting especially the work done by Dr. Braid and Dr. Liébeault. Then he describes the chief phenomena, anaesthesia, rigidity, supersensitiveness, negative hallucination, post-hypnotic suggestion, and so on, from the point of view of the Nancy school. Upon this follows a brief special consideration of the freedom of the will as illustrated by hypnosis. In this state we have the mind practically reduced to a state of automatism. Some amount of self-control, however, remains; and the suggestion of something entirely contrary to the subject's habits will be opposed. This automatism, too, is related to more normal instances of the control of one will over another. In the main, none the less the phenomena of hypnotism show how intimately the question of responsibility is connected with physical conditions, and how easily a condition of irresponsibility is induced.

Ueber hypnotische Suggestionen, deren Wesen, deren klinische und strafrechtliche Bedeutung. JOH. G. SALLIS. Berlin, 1888, pp. 53.

This very commendable pamphlet has for its object the instruction of the intelligent layman in the apparently mysterious phenomena now exhibited and commented upon all about him. The topics are treated in a plain manner, not exaggerating our present knowledge, and taking sound views on all doubtful points, such as the action of a magnet, of sealed drugs, and so on. The three stages of Charcot are not adhered to, and the position approaches that of the school of Nancy. The symptoms are treated under the heads of sensory, motor, and vegetative; the last including the influence upon involuntary organs. Many illustrative cases are cited, particularly in the therapeutic portions. On the whole the pamphlet is very similar to that of Obersteiner, though differing from it in many details.

Hypnotism or Mesmerism. CHARLES B. CORY. Boston, 1888, pp. 61.

America has as yet taken a comparatively small share in the discussion of hypnotism, now so all-absorbing in France and elsewhere. The present volume is a contribution from the chairman of the committee on Hypnotism, of the American Society for Psychical Research. It contains matter already published or read, and is intended to enlighten the American public on a topic in which much misconception exists. A general paper on hypnotism, partly historical and mainly expository, is followed by the most valuable of Mr. Cory's papers, in which he shows, by ingenious experiments, the part played by the voluntary yielding of the subject in the act of hypnotization. When, unknown to the subject of the experiment, he makes the most intense efforts to will her to sleep, no result is effected; while, if the patient is informed that Mr. Cory in another room is willing her to sleep, the desired effect takes place. Mr. Cory concludes: (1) that hypnotism is related to an abnormal constitution of the nervous system; (2) that only a small percentage of persons are hypnotizable; (3) that the condition is entirely due to suggestion, no one being hypnotized without being informed or led to suspect that he is to be an object of experiment; (4) that the condition may be self-induced; (5) that in certain cases the hypnotic is insensitive. Quite as interesting are Mr. Cory's experiments on negative hallucinations, proving most conclusively that the subject takes some accidental peculiarity as the clue to not seeing a certain object, and that if that clue be removed the suggestion fails. The final paper illustrates the therapeutic value of hypnotism.

Ueber Beziehungen zwischen Hypnotismus und cerebraler Blutfüllung. HANS KAAN. Wiesbaden, 1885, pp. 35.

To the support of the vaso-motor explanation of hypnotism, the author brings two experiments, frequently repeated, and corresponding plethysmographic tracings. The only useful subject was a neuropathic woman of twenty-one years. If, when she was in the lethargic stage, a hot compress was applied to her head, she at once awoke. A cold compress had rather the opposite effect. But when she was cataleptic, the hot one was without effect, and the cold one caused a return to the lethargic state. The tracings showed a somewhat increased volume of blood in the arm in the lethargic stage and decreased in the cataleptic. These hot and cold applications, he believes, must have worked reflexly on the vaso-motor system, producing anaemias and hyperaemias in the cortex and lower centers. The author is not inclined to generalize from his single case, and with present information as to the subtlety of suggestion and the perceptive powers of subjects, would realize an alternative to the vaso-motor hypothesis.

Ueber hypnotische Erscheinungen. Vortrag von TH. MEYNERT. Wien. klin. Wochenschrift, Vol. I, Nos. 22, 23, 24 (1888).

Clinical observation and not experiment is, in Prof. Meynert's opinion, the true path to right understanding of hypnotism, and, though with much hesitation, in handling such a subject, he presents two cases from his own observation. In one, the patient had natural seizures, somewhat resembling states of hypnotism; in the other, the subject had been worked up by previous manipulators to a state of ultra-susceptibility. In the states of these two he finds a partial correspondence to the stages of Charcot, and explains what he finds, together with some of the more common hypnotic phenomena, on the basis of circulatory changes in the encephalon, local anaemias and hyperaemias, thus joining himself to an early view of Heidenhain, and one more recently supported by Kaan and others.

Magnetismus, Hypnotismus, Spiritualismus. DR. GEORG V. LANGSDORFF. Berlin, 1889.

The trend of this pamphlet is spiritualistic and unscientific.

Ein Beitrag zur therapeutischen Verwerthung des Hypnotismus. ALBERT, FREIHERRN V. SCHRENCK-NOTZING. Leipzig, 1888, pp. 94.

From this pamphlet one gains an admirable idea of the extensive activity now absorbed by studies in hypnotism. The main portion of this thesis is devoted to a résumé of recent contributions to the therapeutic aspects of hypnotism in various countries. The two longest sections are naturally devoted to France and Germany, but Belgium, Holland, Italy, Spain, England and America, Greece, Poland, Russia, Norway, Sweden, Denmark, Switzerland, Hungary, Austria, are all represented. The point of issue between Paris and Nancy forms a chief point of discussion, the tenets of each school being very clearly and fairly defined: the author deciding against the Paris views. But he is inclined to sympathize most strongly with the views of Fontan and Ségard, who, while siding in the main with the "suggestionists," leave some room for purely physical effects in the deeper stages of hypnosis. To the French belongs the credit of introducing new views into scientific circles, and studying strange phenomena with industry and enthusiasm: to the Germans the credit of introducing rigid criticism, severing the essential from the accidental, and bringing order into this young science.

Much space is devoted to statistics of cures by hypnotism. The large percentage of successful treatments guaranteed by so many who have used hypnotism leaves little doubt of its therapeutic importance. Its influence is most marked upon nervous diseases, though by no means confined to them; but Dr. Schrenck-Notzing is careful to add that it should not be used until other methods fail, and that it should be exclusively in the hands of experts. The motto prefaced by the author to his work might be taken as the watchword of the Nancy school; it reads "*Possunt quia posse videntur.*"

The author contributes reviews of hypnotic literature to the October, November, December, and January numbers of *Sphinx*. J. J.

Neue Heilmittel für Nerven. I. N. von Nussbaum. Breslau, 1888, pp. 16.

An address of rather miscellaneous content, describing various innovations in the hygiene of nervous ailments, massage, nerve-stretching, nerve-vibration, Swedish movements, and so on. Much of it deals with hypnotism as a therapeutic agent; the author rather indiscriminately choosing his facts, and incorporating much that is not at all certain along with the well ascertained. It is merely a popular presentation, lacking all originality.

Der Hypnotismus und seine Verwerthung als Heilmittel. Dr. E. Mendel. Die Nation, Jan. 14, 1888.

Dr. Mendel very briefly records his adherence to the Nancy school, cautions against a too free use of hypnotism as a curative agent, and points out the close analogy of hypnotic suggestion with more normal phenomena. Its curative effects stand on the same plane as all other psychic cures, should be as sparingly used, and retained in professional hands.

Origine des effets curatifs instantanés de l'hypnotisme sur les maladies chroniques. J. Delbœuf. Revue de l'Hypnotisme, Sept. 1888.

The point of the explanation put forward by Prof. Delbœuf is this: the first success in preventing a crisis by hypnotic suggestion acts as itself a suggestion against the next crisis, and that against the next, and so on, the force of the suggestion growing, as is common in hypnotic experimentation, with each success, till at last it is sufficient to overcome a strongly intrenched disease.

De l'auto-suggestion en médecine légale. BUROT. Revue Philosophique, Jan. 1889.

To the question of how to get at the suggester of crime when he has covered his tracks by suggesting amnesia as well, Dr. Burot makes the following contribution. He has found in a number of cases that if a subject, that has thus been made to commit a crime, is taught to hypnotize himself (auto-suggestion), with a view to recalling the forbidden circumstances, he finds himself free of the hindrance and able to do so, can relate the circumstances and identify his principal.

Casuistische Mittheilung auf den Gebiete der Suggestions-Therapie. Dr. E. BAIERLACHER. Münchener med. Wochensch. XXXV, No. 39, Sept. 25, 1888.

The author has tried suggestion upon 58 of his patients, 24 men and 34 women, between the ages of 16 and 71. 7 of the men and 8 of the women proved unhypnotizable, a considerably larger percentage than Bernheim's, but due partly to unfavorable circumstances. He records therapeutic failures in traumatic neuralgia, traumatic neurosis, neuralgia of both legs, apoplectic hemiplegia, persistent insomnia in emphysema of the lungs, and in extreme dizziness; successes in colicky pains following abortion, catarrh of the stomach, occipital neuralgia and neuralgia of the second branch of the trigeminus, irregular and painful menses (a case of each), and muscular rheumatism, besides minor ailments. In two cases he brought about a change of the pulse rate during suggestion, from 92 and 86 to 76; with a consumptive having a pulse of 120 he was unsuccessful.

Der Hypnotismus in der Geburtshilfe. JOH. G. SALLIS. Separat-Abdruck aus "Der Frauenarzt," 1888.

The author relates three cases, previously reported by others, in which hypnotism has been applied in first accouchements. All the patients had been hypnotized many times before the occasions in question, the first two with a view to trying hypnotism as an anaesthetic in labor. From the three it does not appear that hypnosis materially affects the regular course of parturition. In the first and third cases there was external evidence that the pains in the severest phase were actually felt, and in the second the patient was repeatedly awakened by them. The recollections of the pain were destroyed for the normal state. The author adds a brief bibliography of German works on hypnotism.

Dr. Van Renterghem, who, with Dr. Van Eeden, has opened an institute for the cure of nervous diseases by hypnotism, at Amsterdam, has gathered some statistics relative to the curative effect of the treatment. From May 5 to August 9, 1887, 178 patients were hypnotized; only 7 proved complete failures, and 20 became somnambulic at once. 162 were treated, of whom 91 were cured, 46 improved, and 25 unimproved. 37 different diseases were represented, of which the following is a selection:

	No. treated.	Improved.	Cured.	Not improved.
Rheumatic pains, ...	16	2	13	1
Various hysterical attacks, .	24	7	14	3
" neuralgias, ...	9	2	6	1
Epilepsy, ...	3	3	..	..
Indigestion, etc., ...	12	2	10	..
Deafness, ...	11	7	1	3

A review by Dr. Van Eeden, of the Swedish work of Dr. Otto G. Wetterstrand, *Om hypnotismens anvandande i den praktiska Medicinen*, in the *Revue de l'Hypnotisme*, November, 1888, gives some further statistics of the application of hypnotism in medicine. The results accord with those of the Nancy school, to which Dr. Wetterstrand belongs. From January, 1887, to the date of writing, 718 persons had been tried, of whom only 19 were found completely unhypnotizable. Special nervous condition and sex were not found to determine susceptibility ; but character and especially age were important. Children are most easily influenced, and after thirty, susceptibility declines. This agrees with Dr. Van Eeden's experience, though he still believes that in very advanced age the sleep can easily be produced. No serious troubles were found to follow hypnosis, and unpleasant sensations that sometimes did follow could be put out of the way by suggestion. The long list of diseases treated contains many of an organic nature. A beneficial effect is alleged in the hemorrhages and diarrhœas of consumption, in heart disease (the improvement of the heart's action appearing in the tracings), in anæmia, and in Bright's disease. Though the last is also not without parallel in the experience of Dr. Van Renterghem and Van Eeden, the latter is still of the opinion that the treatment is most applicable in functional neuroses. Out of 74 cephalalgics, 65 were cured ; of 26 stammerers 10. The least benefit followed in epilepsy, tabes dorsalis, grave psychoses and neurasthenia (with the last the Dutch doctors have been more successful); some benefit followed in *petit mal* and slight alienation, and much in alcoholism ; in *enuresis nocturna* the success was constant.

Der Hypnotismus in der Pädagogik, von einem Schulmanne. Mit einem Vorwort von Joh. G. Sallis. Berlin, 1888, pp. 17.

This pamphlet, written by a well-known philologian, whose name is withheld, is prefaced by a few words from Dr. Sallis, supporting the conclusions therein maintained, and arguing great caution in introducing hypnotism as a reformatory measure into the schoolroom. He emphasizes the dangers to health and morality likely to accrue from an indiscriminate hypnotization ; urges that in as far as it is an advisable process, pedagogy has other and better substitutes for it, and its only legitimate field is in the case of abnormal children, who at best need a special kind of training. In the preface Dr. Sallis hints at cases in which children have learned to hypnotize one another and use such power in the furtherance of vicious habits. Apart from a somewhat characteristically German opposition to a French proposition, this attitude towards educational hypnotism seems prompted by a wise conservatism and proper caution in dealing with new tools.

Emploi de la suggestion hypnotique pour l'éducation des enfants et des adolescents. LIÉBEAULT. Revue de l'Hypnotisme, Jan. 1889.

The answer to such questions as that of the applicability of hypnotism in pedagogy and moral reform lies in the pooling of observations. To that end Dr. Liébeault contributes a brief statement of 22 cases from his own experience. The subjects ranged in age from 14 months to 19 years. All except one were inferior in intellect, morality or emotional control; 13 were in good physical health, and 8 not so. Of the 13, three were set right, 7 were helped and 3 not helped (7 of these have not yet been long under treatment); of the 8, seven were set right and one not helped. A single case in normal physical and mental condition, a lycée student of sixteen, was able to work more and better while the treatment continued. In all, there were 10 cures, 8 betterments and 4 failures. Hypnotic suggestion proved useful in removing excessive fearfulness in 3, passion in 2, depraved habits in 4, lying in 1; two unruly children were somewhat though not completely restrained. An effort was made some time ago by M. Félix Héments to obtain official permission to put the thing to test on a grand scale among the inmates of the houses of correction. The results of such an experiment would be awaited with the greatest possible interest.

In an earlier number of the same journal (November, 1888), Dr. Aug. Voisin contributes the case of a youth of sixteen who from his boyhood had been uncontrolled, a liar and a thief, and had gone from bad to worse as he grew up. After exhausting his family and several reformatories, he was finally sent to Dr. Voisin. His bad habits gradually yielded to suggestion in successive hypnotizings, and at the end of a month he was apparently reformed. At the end of three months, six weeks after the last hypnotizing, he had had no relapse.

Des hallucinations négatives suggerées. Dr. BERNHEIM. Revue de l'Hypnotisme, Dec. 1888.

That a negative hallucination was a purely psychical act, a refusal to give audience to a sensation received and registered by lower centers, has long been an accredited doctrine of the Nancy school. Perhaps no more striking evidence in its favor has been furnished than that supplied in this article. The subject was a young lady of eighteen years, affected with sciatica, but otherwise without neurotic taint, easily hypnotizable and showing all the characteristic phenomena. Dr. Bernheim gives her the post-hypnotic suggestion that upon awakening she will not see him; he will be gone. Upon awakening he calls her, stands before her, sticks a pin into her skin, but she refuses to recognize any sensation emanating from him. It may be noted that this does not take place with every subject; some will simply not see the doctor, but will hear him and feel his touches; if such are told that they will neither see, hear nor feel Dr. Bernheim, the result is as that described. Desiring to see how far this condition admitted of abuse from a medico-legal point of view, Dr. Bernheim addressed insulting words to her and threatened violence. Ordinarily very sensitive and reserved, she gave no sign of feeling. She was then hypnotized and told that upon reawakening Dr. Bernheim would be there. This works well and Dr. Bernheim begins to question her. She stoutly denies his having been present before, and seems to remember nothing of it. By dint of repeated and insistent declaration that he was present she at last remembers; then remembers what he did; and with great hesitation and blushing gives an account of what happened. This experiment shows that this negative hallucination is purely psychic, the impression being received and registered. It shows, too, that this apparent amnesia can in some cases at least be overcome and the latent, ignored impression forced into consciousness. Furthermore, it brings to light a condition of medicolegal interest: for here, in an apparently normal waking condition, a patient may be insensible to a suggested maltreatment.

Sur l'explication fournie par M. le Dr. Bernheim des hallucinations négatives suggérées. J. DELBŒUF. Revue de l'Hypnotisme, Jan. 1889.

The explanation of Prof. Delbœuf differs from that of Bernheim in the greater prominence that it gives to the co-operation of the subject. He holds, and fortifies his position by citing experiments that the subject behaves exactly as a waking person might that had determined to play the part to the letter. The case given by Bernheim shows nothing that might not have been done by such a person. "The subject lends himself to what is required of him with passivity, but with intelligence."

Recherches sur l'anesthésie hystérique. A. BINET. Comptes Rendus, CVII, p. 1008, Dec. 17, 1888, and Revue de l'Hypnotisme, Jan. 1889.

While skepticism is natural as to the total exclusion of suggestion which Dr. Binet alleges, his experiments are interesting from the likeness of his results to those of experiments on negative hallucination. The negatively hallucinated have been shown to see, hear, smell, etc.; it appears that these hemi-anesthesics really feel. The subjects were twelve hysterics in different Paris hospitals. The following are among the findings reported. Stimulation of an anesthesic area, which the subject was not allowed to see, produced no tactile or muscular sensation, but, instead, a visual image of the area. This could be projected on a screen, and lasted while the stimulation lasted. A prick was seen as a dot; figures drawn with the compass-point appeared in color; gentle constrictions of the wrist or finger called up the image of the part; passive movements were perceived as movements of the image, and could be counted. When two compass-points were applied, one or two dots were seen according to the separation of the points. Measured thus, the discriminative sensibility was found to be about normal. The separation of the points was correctly estimated by some patients if it did not exceed 2–3 cm., but larger distances were underestimated. The shade, light or dark, of the images varied with the subject; the color also changed as sensitive areas were approached. The images behaved in several particulars like after-images. Their details were clearly seen, but sometimes the image did not represent the part; for example, a passively moved finger might be seen as a baton or column. Familiar objects placed in the hand could be recognized as images on the screen. The patients are said not to have suspected the origin of the images, nor to have lost faith in their own anesthesia.

Auto-suggestione in sogno. **Lombroso.** Archivo di Psichiatria, Vol. IX, fasc. 4, p. 418.

In this brief note Lombroso gives the following case in confirmation of the likeness of normal sleep and hypnosis. He had frequently treated a certain neuropathic subject for severe neuralgia; the relief, however, not outlasting forty-eight hours. On one occasion, after suggestion against a painful toothache, the relapse fell in the night. The patient had, however, no sooner fallen asleep than he dreamed of seeing Lombroso, and of having suggestion made to him as usual. The pain ceased, and the relief persisted, like that secured in hypnosis, into the waking state.

Reaction-time in the Hypnotic Trance. **William James.** Proc. Amer. Soc. for Psychical Research, Vol. I, No. 3, Dec. 1887.

These experiments serve to show rather the variety of conditions that may exist in the hypnotic trance than to establish any law of the effect of hypnotism upon reaction-times. Two of the three subjects reacted more slowly in the trance than before it; one reacted in two series a little quicker, and in one a good deal slower. In three series, one on each subject, taken five or six minutes after waking, two showed a quickening even over the reactions taken before the trance; the other showed the same, but, unfortunately for generalization, relapsed into trance again before the set was complete. The average error appeared larger in the trance than in the waking state.

Bibliographie des Modernen Hypnotismus. **Max Dessoir.** Berlin: Carl Duncker's Verlag, 1888. 94 pp. 8vo.

This is a valuable and satisfactory bibliography of the whole subject of hypnotism. The books are entered in the bibliography with their full titles, and the articles have volumes and pages indicated. The division of the materials classified is as follows: I. General, 191 titles; II. Medical, 199; III. Magnetism and Hypnotism, 36; IV. Physiological, 62; V. Psychological and Pedagogical, 85; VI. Jurisprudence, 43; VII. Action at a Distance, 81; VIII. Modern Mesmerism, 58; IX. Various Topics, 46. The last including Preconditions of Hypnotism, Single Cases, Ecstasy, Hypnotization of Animals, Historical works, Theory of Hypnotism, and Hypnotism and Religion.

The statistical tables at the end show: 801 writings, 481 authors, 207 periodicals containing articles.

Of the writings (excluding translations) there are, in French, 473; in English, 102; in Italian, 88; in German, 69; in Danish, 22; in Spanish, 16; in Russian, 12, and from one to six in various other European languages. The growth of interest in the subject is exhibited in the following data: In 1880, 14 writings appeared; in 1881, 39; in 1882, 39; in 1883, 40; in 1884, 78; in 1885, 71; in 1886, 131; in 1887, 205; in 1888 (from January to April), 71. The work is concluded with a full index of authors. In co-operation with Dr. Bérillon, the bibliographical work is to be continued in the *Revue de l'Hypnotisme.* Portions have already appeared in the October, November, and January numbers. D. J. Hill.

III.—EXPERIMENTAL.

On the Seat of Optical After-Images.[1]

M. Binet, in his "Psychologie du Raisonnement" (p. 43 ff.), quotes from M. Parinaud an experiment which, he thinks, proves the cerebral, instead of the retinal, seat of optical after-images. The experiment is as follows:

"Close and cover with the hand the left eye, and with the right eye fixate carefully for some moments a small square of red or black paper on a white background. If now the square of paper be removed, as by blowing it aside, a negative after-image of it will be seen by the right eye on the white ground. Now at the moment of blowing aside the square, close and cover the *right* eye and open the *left* one. After a moment the white field will darken, and on it will be seen the negative after-image."

This formation of an after-image in one eye and its subsequent appearance on the field seen by the other is taken by M. Binet as complete proof that the after-image lies not on the retina, but in the common cerebral center of vision for both eyes. Thence it is projected into the field seen by whichever eye is open. But he quite overlooks the possibility that the image lies really on the right retina and mingles itself with the field of the left eye. He takes it for granted that the phenomenon is seen by the eye which is open, and not by the closed one. That he is wrong in his deductions, and that in failing to consider the possibility above mentioned he has missed the true explanation of the phenomenon, it is the purpose of this note to show.

A serious difficulty in settling the question lies in the well-known impossibility of separating the visual fields of the two eyes. Whether one eye or both are open, whether they are focused on the same point or are held parallel, or squinted, or even jammed into all sorts of relative positions by fingers inserted into their sockets, the field of each will appear to coincide with the field of the corresponding portion of the retina of the other. If an after-image be formed on both together, one image only will be seen, whatever their relative positions; and if the image be formed on one alone, it will yet be seen in the corresponding portion of the field of the other, provided the brilliancy of the second field be not so great as to obscure the much weaker sensation of the image. In reality, in this experiment the after-image never does appear on the left field until the latter has so greatly darkened as to allow it to be seen; and in the periodical increases in brilliancy of the left field the image disappears.

It will thus be seen that a retinal seat of the after-image explains all the facts as easily as does a cerebral seat. Hence the assumption of the latter by M. Binet and others is entirely superfluous. We now hope to show that it is not merely superfluous, but impossible.

Although we cannot so separate the fields of the two eyes as to determine to which of the two the image belongs, yet we can determine that it does not belong to both. This may be done by observing the different effect on the image in interfering successively with the field of each eye. If the location of the after-image is in the cerebral sensory center, we should naturally suppose that through whichever eye it may be seen it will present the same characteristics. If it does not, this must be due to disturbances persisting in the right retina or optic nerve. But if this be once granted it will easily be seen that these persisting disturbances in reality alone account for the after-image and all its phenomena. It is curious to note that M. Binet also mentions the differences in action of the two eyes, but without once thinking of this most natural explanation. The principal differences observed are these:

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[1] Read at the Graduate Course in Psychology at Harvard College, January, 1889.

1. If after obtaining the after-image the right eye alone is opened, the after-image is seen at once on the white ground. If, however, the left eye alone is opened, the image does not appear until the left field is so darkened as to allow the image on the right retina to assert its presence. If now at this point the right eye is again opened, it will be found that its after-image is still plainly visible.

2. After obtaining the after-image, close both eyes. If now the left eye be opened, the image is dimmed or blotted out; and brightened when the eye is again shut. If the right eye alone is opened, the opposite effect is experienced; when the eye is open the image is brighter, when shut it is dimmed.

3. Leave now both eyes open after the image is obtained on the right one only. Thrust before the left eye a finger, pencil, or other opaque object, and no change (except a possible increase in brilliancy) will be observable in the after-image. If, however, the object be placed before the right eye, the image disappears temporarily and does not reappear, if at all, until the right retina is newly fatigued by its new background.

4. The following corroborative observation is due to Mr. E. C. Sanford: "If I brand a strong after-image on my right retina and project it on a white surface, I see it green; if I close the eye and project it on the dark field, I see it rose-colored. Now if I project it (as it seems) with the left eye (right eye closed, left open), I see it not green, but rose—exactly the tint in the closed eye, as I can see by quickly closing the left eye. I see it projected against the dark field—in other words, with the covered eye." In verifying this observation, I have found the following to be true in my own case: After obtaining the after-image from a red square of paper, if I project it on a white surface it appears *light* green; if I close both eyes and cover them thickly in such a manner as to shut out all external light, the after-image appears a much *darker* green; if, however, I merely close the eyes without covering them, so that considerable light can enter from without through the eyelids, the image then appears light rose, light blue, etc., according to circumstances. This affords a very conclusive test. I obtain a strong image with the right eye, then close and cover it, and open the left eye. Soon the left field darkens and the *dark green* after-image appears; this gives place to the *rose-colored* image if the covering is removed from the right eye and the eye kept closed; and this in turn to the *light green* if the right eye is opened. These three colors can be made to succeed one another indefinitely without in any way interfering with the open left eye, which alone, according to M. Binet, is the source of all the visual impressions present!

5. Retinal rivalry has been suggested by Prof. James as a test, and I also have found it reliable. After obtaining the after-image with the right eye, look at the background through colored pieces of glass, using both eyes simultaneously, but each looking through a

color different from that of the other; or by means of parallel vision, look with the right eye at a background of one color, with the left at one of another color. When the color of the left field is predominant, no after-image will be visible; when the right predominates, the image will be seen upon it.[1]

In the above experiments it will be seen that whatever be done to the left eye, the after-image suffers no change, except that of being brightened by diminishing the brilliancy of the left field, and dimmed or destroyed by increasing it. But whenever the right eye is interfered with in various ways, the image suffers corresponding modifications. These differences cannot be explained if we suppose the seat of the after-image to be cerebral. They are all easily explainable if the seat is retinal. M. Binet and others have made the great mistake of supposing that whatever they might see in the field of the left eye when it alone is open, is seen by that eye. That this is not necessarily so, and that in the phenomena presented by this experiment it is not possibly so, has been shown by the above facts.

EDMUND B. DELABARRE.

Energy and Vision. S. P. LANGLEY. Am. Jour. of Science, 3d Series. Vol. XXXVI, p. 359.

Professor Langley has made a fresh determination of the brightness of the different portions of the spectrum. The investigation was made with great care, with a high sun, and errors from all possible sources were calculated and eliminated. The method chosen, after trial of others, was to determine how far away a screen carrying a portion of a table of logarithms had to be pushed in order for the figures to be just legible. The light from a slit, after passing through a collimating lens and a prism, was received on a silvered concave mirror, which formed a spectrum 90 mm. long in front of a second slit. Any color could be made to pass through this slit by setting a graduated circle; it then fell upon a black screen, through a hole in which, 1 cm. square, the table of logarithms was visible. The room was absolutely dark, and the position of the screen was got, by feeling notches, to within a centimeter. A variation of intensity of 225 times was had by the sliding screen alone. By changing the first slit and by introducing a photometer wheel, a variation a thousand times greater could be obtained. The selective absorption of silvered glass had been before determined by an ingenious method, and was now allowed for.

The results obtained are not easily compared with those of former observers (without making a graphical construction), for Langley's observations are taken at every .05 micron of wave-length, and those of Frauenhofer and Vierordt (the ones usually referred to) at Frauenhofer lines; but it seems plain that Professor Langley himself and his other three observers differ much more from each other than from Vierordt. The following table gives the results (for the non-normal spectrum) for Prof. Langley, another observer (*a*), and Vierordt, the two former reduced to 1000 for yellow, for the sake of the comparison:

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[1] It will be interesting to apply the following as yet untried test when a sufficiently sensitive hypnotic patient can be found who can obtain good after-images, but who has himself no theory as to "cerebral" or "retinal" seats. Let him obtain with the right eye a strong after-image, and then by suggestion paralyze completely the sight of that eye. If, then, no after-image is seen with the other eye open, it will prove that the cerebral center has nothing to do with the production of the image; if, however, the image is perceived, it will merely indicate that the paralysis of the right optic nerve has not been complete, and the experiment will have proved nothing.

	45	50	55	60	65	70
(*a*)	151	156	1000	996	208	16
V.	128	370	1000	780	128	22
L.	64	774	1000	141	13	2

These values of Vierordt are for points very near the wave-length at the head of the columns. Vierordt's method was to measure the amount of white light that had to be added to a given color to make the color undistinguishable. Capt. Abney's curve agrees closely with Prof. Langley's.

Prof. Langley has made use of his admirable determination of the distribution of energy in the solar spectrum to obtain the brightness per energy of the different colors. The mean of his three observers, exclusive of himself, is as follows:

Color,	Violet.	Blue.	Green.	Yellow.	Orange.	Red.	Crimson.
Wave-length,	40	47	53	58	60	65	75
Luminosity,	1.6	62,000	100,000	28,000	14,000	1200	1

That is to say, crimson light from the end of the spectrum has to have 100,000 times more energy than green in order to give enough light to enable us to read by it. The absolute work done by that crimson is .001 of an erg, and by the green .00000001 of an erg.

It is not quite plain why Prof. Langley should definitively set down the green as the brightest part of the spectrum, on the testimony of his three observers (he has drawn the curve for only two of them), when he himself, Capt. Abney, and all former observers, beginning with Newton (who says that yellow and orange affect the senses more strongly than all the rest of the prismatic colors together), have considered the brightest part to be in the yellow. Prof. Langley himself suggests that young eyes may be more effective towards the blue end of the spectrum than older ones. C. L. F.

Ueber die Unterschiedsempfindlichkeit des normalen Auges gegen Farbentöne im Spectrum. Dr. W. Uhthoff. Archiv für Ophthalmologie, XXXIV, 4, pp. 1-15.

The sensitiveness of the eye to the change of color produced by a given change of wave-length has been investigated before by Aubert, and more recently by Mandelstamm and by Dobrowolsky. The last two made use of Helmholtz's ophthalmometer without the eye-piece; both plates were lighted up by monochromatic light, and one was then rotated until a difference of color was just perceptible. Dobrowolsky, by means of two Nicols with a quartz plate between them, caused both colors to be of equal intensity, a precaution which is particularly necessary near the ends of the spectrum. They found two points of maximum sensitiveness, one at *D* and one at *F*; the fraction of its wave-length by which a color had to be changed in order to seem changed was three times as great for green as for yellow or blue, and at the ends of the spectrum it was greater still. B. O. Peirce (*Am. Jour. of Science*, Oct. 1883) obtained a similar curve. König and Dieterici, by the method of mean errors and a different apparatus, got a curve which differs chiefly in rising very abruptly at the two ends; beyond the wave-lengths μ.43 and μ.66 they affirm that absolutely no differences of color are perceptible, but only differences of brightness. Since the method of mean errors is considered by some to be objectionable, Uhthoff has repeated these experiments, using the same apparatus, but the method of just perceptible differences. His results resemble very closely those of the last two observers, but they differ somewhat at the ends. The difference is such as to leave it quite undecided whether it is caused by the difference of method or by the individual differences of the eyes experimented upon. Since the principal object of the investigation was to compare the methods, it is a pity that both methods could not have been applied by the same observer.

Peirce found the eye more sensitive to change in the yellow than in the blue; the reverse of that was found by all the other observers, but that is probably because his curve represents the mean of many different observers. He did not detect the changelessness of the two end-portions of the spectrum, but this, Uhthoff considers, is because he did not make the colors compared equally bright. The fact, if it is a fact, is of great importance for the theory of color-vision, and it would be well if it could be confirmed by other observers. Uhthoff says that the securing of equal brightness is less necessary in the middle of the spectrum than at the ends. This is strange, because, according to Langley's brightness-curves, the brightness is changing most rapidly on either side of the green. Can there be any significance in the fact that the change of brightness and the change of tone are both most rapid in the same part of the spectrum, namely, in the yellow and the blue? C. L. F.

Experimentelle Untersuchungen über die psychophysische Fundamentalformel in Bezug auf den Gesichtsinn. Dr. Arthur König und Dr. Eugen Brodhun. Sitzungsber. der König. Preuss. Akademie der Wissensch. zu Berlin, 1888, pp. 15.

Doubting the applicability of the psychophysic law to light illuminations, these experimenters decided to find the differential threshold with various intensities, from the slightest barely perceptible illuminations up to intense brightness, almost glaring. The light was of six different spectral wave-lengths, 670μμ, 575μμ, 505μμ, 470μμ and 430μμ, these answering to the fundamental colors of their visions. König has normal color perception, but Brodhun is green blind. Their rather intricate apparatus and method of observation need not be detailed; the final problem in each observation consisted in adjusting one rectangular patch of spectral light so that it appeared just noticeably darker than another. The intensity of the illumination was varied for each wave-length from 1 upwards to 2, 5, 10, 20, 50, 100, 200, 500, 1000, 2000, 5000, 10,000, 20,000, 50,000, and 100,000, and downwards to .5, .2 and .1, the light 1 being the illumination seen by an eye looking through a diaphragm one square millimeter in aperture, at a surface coated with oxide of magnesium, the surface standing at a distance of one meter, and reflecting the light from a glowing platinum surface one-tenth square centimeter in area, standing parallel to it. Grouping all the wave-lengths together, it is found that between intensities 100,000 and 20 there is a very regular slight increase in sensibility, followed by an equally regular and slight decrease, the sensibility between intensities 1000 to 20,000 being very nearly the same. The curve for this region would thus be a shallow trough. The average value of the sensibility between 100,000 and 20 is about 1/44. They also conclude (1) that the variations between the two observers are so slight as to show that the differences in their color distinctions do not affect the results; (2) that from the highest intensities down to 100 or 20, the wave-length does not affect the sensibility, the latter being a function of the illumination only; (3) that the three wave-lengths 670μμ, 605μμ and 575μμ form one group, and 505μμ, 470μμ and 430μμ form another group, the sensibility decreasing more rapidly with the latter than with the former. Again, the intensity of the light distinguishable from darkness was, from the longest to the shortest wave-lengths, .11, .011, .0055, .00035, .00013, .00014 intensities respectively. A very important conclusion that the authors draw is that with a brightness *subjectively* the same, the sensibility is independent of the wave-length; in other words, the same physical intensity in various colors does not appear the same psychically; and it is the latter that is the standard, not the former. J. J.

On some peculiarities of the phantom images formed by binocular combination of regular figures. JOSEPH LE CONTE. Am. Jour. of Science, 3d series, XXXIV (1887).

The observations presented by Prof. Le Conte are four. (1) On looking downward at about 45° on a regular repeated pattern like that of an oil-cloth, and combining the patterns by crossing the eyes, the observer sees the phantom plane raised at the adjacent end, the elevation increasing as figures farther and farther apart are combined. The reason is that the rows of figures as they run forward from the observer slant inward (except the middle one) by mathematical perspective. As lines of figures with greater and greater slant are made by the crossing of the eyes to serve as the middle one of the field, they cross at greater and greater angles. Since this is what would actually be present if the observer were looking down a sloping plane, the image is involuntarily interpreted after that analogy. The individual figures appear elongated because referred to such a plane. The reverse effect is here as elsewhere to be obtained by combining the images with parallel lines of sight. (2) If a vertical plane is taken, on looking upward or downward the image inclines away in the direction of sight. (3) If the eyes are kept at the neutral point (about 7° above the horizontal for Prof. Le Conte himself), no such sloping is seen (if anything, the reverse curvature), but the plane falls away at the sides. If, however, the eyes are moved upward and downward, both curvatures appear. The sides are seen sloping because the points of the real plane to the right and left of the fixation point are represented on retinal points that belong to homonymous images, and are therefore interpreted as beyond the fixation point. (4) If the vertical surface is bent on its vertical diameter, like a half-open book seen from behind or before, the convex (or concave) effect is increased, as with the Le Conte Stephens stereoscope (Am. Jour. Sc. 3d series, XXIII, 297 ff.), but this increase is due to geometrical, not to retinal causes. The curvature of the plane in the third observation is a corollary of the circular form of the horopter in such a position of the eyes.

Neue Versuchungen an den Ohrenbogengängen. J. BREUER. Pflüger's Archiv, Bd. XLIV, p. 135, 1888.

Any doubt which may have remained among physiologists as to the effect of the semicircular canals upon the sense of direction must be completely set at rest by these admirable experiments of Breuer. The one thing which remained to be done—the separate excitation of the three canals by the electric current, and the production of the corresponding motions of the head under circumstances which excluded the possibility of any accompanying injury to the brain—has now been successfully accomplished. These experiments are, of course, very much superior in delicacy and conclusiveness to those in which the semicircular canals are cut. As little as possible of the ampullae is exposed, and the dove is kept in a state of stupor sufficient to prevent spontaneous motions. The strongest reactions—motions of the head through an angle of forty-five degrees—are got by the application of heat and cold. As Breuer showed in a former paper (1875), the motion, in the plane of a given canal, is in a different direction according as one end or the other of its ampulla is excited. When the electric current is used, the direction of motion changes with the direction of the current, and with the intermittent current no motion at all is obtained. The head of the dove may thus be made to move at pleasure in any one of six different ways, two in each of the three planes of the semicircular canals. That the current does not act directly upon the brain is proved in the following way: the point of the gold-tipped needle which forms the cathode is first inserted into the brain near the ear, and then the strength of the current is diminished until motion is no longer obtained in this way. The needle is then applied to the canals, and the same strength of current is here found to be sufficient to produce a marked reaction. It is not, of course, shown that the cerebellum is not concerned in the motion, but that the canals are the peripheral sense-organ for the centers in the cerebellum. Breuer's former experiments in mechanical stimulation of the canals he has repeated and confirmed. Motion of the head can be made to take place in one direction or the other according to the end of the canal from which the endolymph is sucked out by a scrap of blotting-paper; the direction of the motion is the same as that of the endolymph-stream.

It would seem that a more important rôle ought to be attributed to the semicircular canals in the derivation of the space-feeling than is usually done. It may also be conjectured that a fourth dimension in space will remain forever inconceivable to us until after we have developed a fourth semicircular canal. C. L. F.

Die Abhängigkeit zwischen Reiz und Empfindung. Dr. JULIUS MERKEL. Philosophische Studien, IV, 4, pp. 541–596, and V, 2, pp. 245–292.

This elaborate paper recounts, with a mass of unnecessary details and a bewildering abundance of confusing tables, a series of experiments designed with great care, carried out with infinite patience, and directed to the solution of the most important problem of psychophysics. It would be impossible to notice here all the many points touched upon in this comprehensive study; only the conclusions reached, the methods used, and the inferences drawn from the results can be summarized. Ordinary observation would call attention to the fact that sensation increases more slowly than stimulation; double as large a chorus does not give double the effect, and so on. But what the ratio between the two is remains to be determined. The logarithmic ratio is only one of a number. Moreover, the results by one method will not be directly translatable into another. Only by the agreement of several methods applied to the same problem can a conclusive result be reached. The main issue will be between the hypothesis claiming the distinctions between sensations to depend upon relative increments of stimulus, and that claiming that they depend upon absolute increments. The methods that Merkel employs are those of the just observable difference, the method of doubles, and the method of mean gradations. The first is used in a novel and improved form. It consists in asking the subject to adjust a stimulus so as to give a just appreciably greater sensory effect than a given stimulus; in other words, to record the difference at which a confidence nearing certainty is reached. The method of doubles consists in setting one stimulus so as to give a sensation twice as intense as a given sensation. The inference from the method is, that according to the one hypothesis the stimulus should be more than double, while according to the other it ought to be approximately correct. The third is a well-known one of Wundt's, and consists in adjusting a stimulus to give a sensation midway in intensity between a given pair of sensations. The argument is that if the "absolute" hypothesis holds the result would be the geometrical mean, while if the "relativity" hypothesis holds it would be the arithmetical mean. The two senses experimented upon were the sense of visual brightness and that of pressure with an admixture of the muscle-sense. For the former, three parallel dark chambers, each containing a lamp, were so arranged as to illuminate three disks with light of variable intensities. The adjustment was made by moving the lamps toward or away from the observer, and many precautions were taken to insure accuracy in the comparisons. The effect of contrast between the disks is the most disturbing factor in the experiments, and is compensated more or less completely by various devices. Calling the weakest illumination the apparatus could give 1, the range was from 1 to 4096.

The chief results are the following: (1) The method of the just observable difference shows Weber's law to be not valid between light intensities .5 to 64 (the sensibility increasing with the stimulus), but from 64 to 4096 the law is approximately correct; (2) the method of doubles shows that a stimulus is regarded as the double of another before it is really double, and this probably as the result of contrast; (3) the method of mean gradations shows a result much nearer the arithmetical than the geometrical mean; (4) the inference is that if two stimuli are to be distinguishable, the resulting sensations must bear a constant *ratio* to one another; (5) accepting this last hypothesis, one may say that if the stimulus .5 be regarded as completely converted into sensation, only .17 to .3 of the stimuli 64 to 4096 can be regarded as thus converted; (6) if the effect of contrast be eliminated in the method of doubles and of mean gradations, the results are in harmony with those of the just observable difference under the acceptance of the "relativity" hypothesis.

The pressure apparatus consisted, in essence, of a beam with a weight variable in size and in its position on the beam; the short end of the beam was provided with a cap upon which the finger pressed down, thus supporting the weight attached to the other end. This downward pressure is evidently a muscular effort, even though the movement was only a few millimeters; and the resulting values seem to indicate a greater sensibility than the pure pressure-sense would give. By having three such beams, all three of the methods used with visual impressions could be applied to this kind of touch sensations. The pressures were varied from 1 gramme to 2000 grammes. The results expressed, as those with sensations of brightness, are as follows: (1) The sensibility increases as the stimulus increases up to about 200 grammes, and from there to 2000 grammes is quite constant. (2) The sensibility is finer (*a*) with successive than with simultaneous impressions; (*b*) when muscular sensibility is added to pressure sensations, than without the latter; (*c*) when the same finger is used for the various sensations than when different fingers are used; (*d*) when the surface in contact is small than when it is large—these points holding for all the methods of experimentation as well. (3) In the method of doubles, the ratios assigned as the double decrease as the stimuli increase. (4) By the method of mean gradations, the adjustments are much nearer the arithmetical than the geometrical mean. (5) On the basis of the relativity hypothesis, and assuming that with the sensation of 1 gramme all the stimulus is converted into sensation, then from 200 to 2000 grammes only .114 to .163 of it is thus converted; and a not very different result is obtainable from the other two methods when the effects of contrast are eliminated.

This research is thus in opposition to several of the accepted generalizations of psychophysics, and though some of this antagonism is more apparent than real, it will be a most delicate and difficult work to bring unity and harmony into this most perplexing field of experimental psychology. J. J.

Ueber den Rhythmus centraler Reize. Dr. R. v. Limbeck. Archiv für experimentelle Pathologie, Bd. XXV, H. 2.

The author has reopened the question of the rhythm of muscular contractions following central stimulation. Using induction shocks and recording the results graphically, he stimulated the cortex in dogs and rabbits and the cord in rabbits and frogs directly, and the cord in frogs, toads, rabbits and doves reflexly, stimulating the N. ischiadicus on one side so as to cause contractions on the other. In contradiction to the hitherto accepted view, he found that the central system did not send out motor impulses at a fixed rate, no matter how fast stimuli were sent into it, but that, within the limits of experiment, as many impulses were sent out as were received. His rates were for the cortex 6½–13 per sec., for the cord 5½–34, and for the same by reflex stimulation 4½–19¼. Faster rates, when applied, gave smooth curves. Tracings of the spontaneous tetanus of strychnine poisoning showed a variable rate of central discharge.

Ein photometrischer Apparat zu psychophysischen Zwecken. A. Kirschmann. Philosophische Studien, V, 2, 1888, pp. 292–301.

Owing to the difficulties in the accurate observation of differences of sensations of brightness, such as contrast, differences in sensibility of neighboring parts of the retina, variations in accommodation, not to mention mechanical difficulties, Kirschmann devoted himself to securing a method of comparing luminosities free from such defects. He lays down two principles: (1) that the two surfaces to be compared must be equally distant from the eye of the observer, and if possible, in the same plane; (2) the two surfaces must be in contact. A tube 20 cm. in diameter, coated inside with lamp-black, and adjustable to a length of 60–100 cm., furnishes a dark chamber. In front of this a rotation-apparatus sets in motion a disk 21 cm. in diameter, just covering the opening of the tube. This disk has two quadrants white; and in the others a band of black or gray, and concentric with it an opening through which one sees into the tube. Both these are regulated in quantity by an adjustable portion of a disk attached over them. The object now is to make the black band equal in intensity to the black of the tube seen through the openings. If the opening is $a°$ wide and the black band $b°$, and the reflecting power of the black is called 1, of the white x, and of the tube 0, then $(360 - a)x = b + (360 - b)x$, or $x = \frac{b}{b - a}$. This is under the supposition that the tube reflects no light, which is not strictly true. If we call the slight light coming from the tube with an opening of $1° = k$, then the corrected formula is $(360 - a)x + a^2k = b + (360 - b)x$. And if in another case the settings are a_1 and b_1 degrees; then $(360 - a_1)x + a_1^2k = b_1 + (360 - b_1)x$; from which the two unknown quantities k and x can be found. So slight, however, is this value of k (estimates make it less than 1/5700 or 1/6800 of the reflection from white cardboard), that it is not detectable in the general result. A comparison of a band of "*Pariser Schwartz*" with white cardboard by this method gave a ratio of 1 to 66.2, which agrees well with 1 to 68, found by Lehmann. Continual use seems to increase the power of reflection of black; making the ratio in one case 1 to 51.2. Similar measurements gave with lamp-light a ratio of 1/60 (for black to white); with gas-light 1/58.2, and with diffused daylight 1/57.2. These differences are probably due to the impurity of the white of the cardboard. The following table of comparison with white cardboard of substances usually employed to produce black may be interesting:

	Lamplight.	Diffused daylight.
Paris black,	1/60	1/57.2
Indian ink,	1/23.6	1/20.2
Indigo,	1/26.8	1/27
Graphite (Faber BB),	1/8.6	1/8.9
" (" B),	1/6.2	1/5.9

This apparatus seems to be satisfactory and commends itself to various uses. J. J.

IV.—ABNORMAL.

De la Paralysie Générale d'origine traumatique. B. BALL. L'Encéphale, July, 1888.

Paralysie Générale d'origine traumatique. L. F. ARNAUD. L'Encéphale, November, 1888.

Prof. Ball reports three interesting cases of general paralysis of traumatic origin, and such cases are so rare as to merit special comment. The first was a postal employé who was thrown against the side of a wagon. His hereditary antecedents and personal habits were irreproachable, and the disease was clearly traceable to the traumatism. The injury was received February 28, 1887, and death occurred from exhaustion, September 16, 1887, giving a period much under the usual duration. Dr. Arnaud's article is an account of the autopsy, which fully confirmed the diagnosis.

The second case was that of a mechanic who was struck by a ball of globular lightning during a severe thunderstorm. In this case, as in so many where the disease is dated from a fixed time, the traumatism appears to have been merely the shock necessary to light up into activity the disease which had existed in a dormant state for some months.

The third case is the most interesting of all; the patient received a blow on the left forearm, injuring the ulnar nerve, and followed by muscular atrophy. Mental troubles came on after the accident, and a well marked case of general paralysis developed. Here the general paralysis was directly due to the traumatism, and the case is of great interest from the exactness with which it was possible to show the origin of the disease. W. N.

Ueber das melancholische Anfangsstadium der Geistesstörungen. LUDWIG SCHIRMEYER. Inaug. Dis., Strassburg, 1886.

The author tabulates statistics of 290 cases (melancholia 98, mania 69, paranoia (*Verrücktheit*) 88, feeble-mindedness (*Schwachsinn*) 20, distraction (*Verwirrtheit*) 15) in order to determine the frequency of a beginning in melancholia. Counting in the melancholiacs, less than one-half of all showed a depressed first stage; throwing out the melancholiacs (except some cases not fully observed), the proportion is only about one-fourth. Taken separately the proportions were, mania about one-third (or excluding certain cases for cause, about one-fourth), paranoia a little more than one-seventh, feeble-mindedness one-fourth, distraction over one-third. The melancholiac terminal stage, defended by some, was found in but few cases. Insanities are often preceded by conditions of ill-feeling, but this is by no means the melancholia of the alienist. The author's figures bring him into agreement with Witkowski, and into opposition with Arndt. The statistics are preceded by an extended summary of previous opinions.

Ueber Bewusstseinsstörungen und deren Beziehungen zur Verrücktheit und Dementia. Dr. J. ORSCHANSKY. Archiv f. Psychiatrie, Bd. XX, H. 2.

The author points out in a group of psychic conditions, variously named by various authors (*Verwirrtheit*, Wille; *la démence*, Esquirol; *acuter hallucinatorischer Wahnsinn*, Meynert; *acute primäre Verrücktheit*, Westphal; *acuter sensueller Wahnsinn*, Schüle; dreamy-condition of different authors) a common element, to wit, a deep obscuration of consciousness, or an "ataxy in the psycho-physic sphere." In a typical case the patient is cloudy in his conception of himself; the bounds of his *ego* and the non-*ego* are obscured; his notions of time and space are uncertain; memory images and acquired associations are weakened or lost; if at times he is able to recognize his surroundings, he is not able to connect the impression of them with similar impressions before received. As a consequence, right perception and logical framing of ideas are out of the question. In the fields of volition and emotion the obscuration is as great. With its depth the expression of it changes from a simple dazed condition to one resembling coma. Upon this simple background may appear illusions, hallucinations, and delusions to such a degree as to color the whole; and since the condition frequently enters only as a stage in a complicated psychosis, it is apt to be still further overlaid by the residua of delusions from earlier stages; whence its confusion with primary dementia and paranoia, the two extremes between which it varies. A typical case begins and ends with periods of disturbance of consciousness passing through a delirious stage between the two, and having a favorable prognosis in proportion as the obscuration of consciousness is deep. For the author, as for Wundt, consciousness has a functional side, and it is by no means simple. It involves the regulative ideas of the *ego*, cause, time and space, etc., and by its complexity makes possible a "change of consciousness as a whole, which to a certain extent is independent of the quantitative and qualitative change of the elements of consciousness, as also of the psychic function, *i. e.* of the framing and working over of ideas."

Klinische Beiträge zur Kenntniss der generellen Gedächtnissschwäche. Dr. C. S. FREUND. Archiv f. Psychiatrie, Bd. XX, H. 2.

The author has observed two cases of pathological weakness of memory. Both were women, one 52, the other 65; both had suffered severe nervous troubles from alcoholic excess, leading up to psychical disturbances, after which the chief remaining symptom was the weakness of memory, complicated in one case by senile dementia. In both the sensorium was clear of hallucinations and sense illusions, apperception little affected, and untroubled by delusions; bodily disease was absent, except in one, whose trouble was tabetic. The memory of things in early life (before 30 in the first, before 20 in the other) was much better than for recent events. The first could not remember her own bed, nor the day of the week, nor, after a few hours, whether or not she had dined. She forgot repeatedly the name and use of a stethoscope that was shown to her and explained; she could only repeat phrases of some length when spoken slowly and distinctly and perhaps repeated. Similarly with tunes, and the first tune of several seemed best remembered. She could multiply numbers, but forgot the examples; she could, however, remember a number over a period of conversation about other things. She showed also illusions of memory. Her power to write was not affected. In the second case, however, this was an interesting feature. In spontaneous writing she repeated words in whole or in part, together with parts of words to follow, before getting the latter written correctly. She had difficulty in recalling the capital letters, though what was at the instant forgotten often returned later. Writing from a copy became almost free of errors when she spelled the words aloud as she wrote. Writing from dictation was worst of all. In the dictated alphabet errors seemed to occur in proportion to speed. The patient could recognize errors, and the disturbances varied with her power of memory. That sufficed for her to spell polysyllabic words orally, but not to write them, and in spelling she was apt to add letters after completing the word. In other particulars she resembled the first case.

Ueber epileptische Amnesie. HERMANN HELMING. Inaug. Dis. Halle, 1883.

Dr. Helming has studied, with special reference to memory, a case of epileptic insanity. The patient had epileptic spasms occurring periodically, showed a suspicious and violent character during intervals of sanity, and in periods of insanity manifested characteristic symptoms of epilepsy, having sense hallucinations, great ideas, religious delirium, and the like. Yet, generally, for some time after attacks of insanity, the patient remembered all that had occurred, except what happened during the convulsions. Also in attacks of insanity he remembered the events of previous attacks. Hence the author concludes that defect of memory, which has been deemed a chief characteristic of epileptic insanity, cannot be relied upon. The ability to recollect what has occurred during an attack does not exclude the possibility of epilepsy. Instead of depending upon a single symptom, more stress should be laid upon the general character of the attack and the epileptic nature of the patient. This thesis contains a brief summary of theories of epileptic insanity, and to it is appended the usual bibliography. W. H. B.

On the Dissolution of the Functions of the Nervous System in Insanity, with a Suggestion for a New Basis of Classification. J. MACPHERSON. Am. Jour. Insanity, January, 1889.

Following Hughlings Jackson, the author looks upon the insanities of maturity as dissolutive. In general this dissolution is shown chiefly in one of the great fields of mental function, emotion, intellect or will, though more or less in all. Dissolution in the first results in melancholia and mania, in the second in the delusional insanities, in the third in insanities of the moral and impulsive kinds, and, since the power of origination depends on will, in dementia.

Alcoholic Heredity. Dr. F. LEUTZ, Medical Director Government Insane Asylum, at Tournai, Belgium. Quarterly Journal of Inebriety, April, 1888.

Dr. Leutz adds nothing new to our knowledge of alcoholic heredity, and his short paper is simply a review of the opinions brought forward by others, but the subject is of such universal importance that it may not be improper to note the two forms that alcoholic heredity assumes. The first is homologous heredity, or that of similitude ; second, the heredity of transformation, or eccentric heredity. In the first form the progenitor gives to the descendant his tendency to alcohol, or symptoms of his alcoholism ; in the second form, the alcoholized mental state of the progenitor becomes transformed into varied nervous disorders. No one denies the direct heredity, but there is but little agreement about the frequency of its transmission. The multifarious forms in which the second class appears, in irritability, instability, and a vicious moral disposition, need not be dwelt upon. W. N.

Proceedings of the Congress of the National Prison Association of the United States, held at Boston, 1888. Edited by the Secretary, FREDERICK HOWARD WINES, Springfield, Ill.

While the Proceedings are naturally and by right mostly taken up with questions of administration and of the improvement of the criminal classes and the possibilities of their reform, there is one subject dealt with here that is of interest to the student of morbid psychology, the report on *The Registration of Criminals* (p. 73). "The new method of identifying prisoners, introduced into France by M. Alphonse Bertillon, and which is now successfully practiced not only in all the French prisons, but also in Russia, Japan, Spain, Italy, and some parts of Germany, has, within the past year, made quite a headway in the United States. It consists in the exact measurement of the prisoner on his arrival at the jail or prison. His height, the length and width of his head, the left foot, the outstretched arms, the trunk of the person seated, the four fingers of the left hand, the left arm, and the length of the ear are measured; and the color of the eyes and any particular marks are noted down; and a photograph of the prisoner is taken, both in profile and full-face view. It will be noticed that all the measurements mentioned are those of the bone dimensions of the human body after the body has attained its mature growth." The measurements thus obtained are catalogued and systematized in such a manner that it is possible to identify immediately, by reference to a central office, any person whose measurements have been recorded. The psychological interest of this new departure is that it promises some light on the existence or non-existence of the so-called criminal type. If such measurements are carried out systematically over a series of years and are then submitted to rigid investigation, the means would seem to be at hand for settling some of the questions raised by Lombroso and others of the new school of criminologists. The practical value of the method for gaining the result sought, the identification of criminals, has been abundantly proved in France, where about one hundred thousand cases are filed, and where they are used every day in furnishing information to the courts; no mistake has as yet occurred. W. N.

Verbrechensverübung im Traumwandeln. Dr. HERMANN ORTLOFF. Gerichtlich-medizinische Fälle und Abhandlungen, Heft II, 1888, pp. 35–64.

The author gives in readable fashion a case of alleged arson that came before him for investigation, in which the question of crime in sleep-walking held an important place. A fire broke out at night in a farmer's house, destroying a large part of its contents, including valuables belonging to his wife. Suspicion fastened on the wife herself, chiefly on the testimony of the maid-servant, who asserted that her mistress was eccentric, went about the house at night, and had brought fagots into the house just before the discovery of the fire. The place where the fire started made it quickly destructive to the property of the mistress, and for other reasons it seemed clear that if she set the fire she did so without consciousness. The assertions of the servant were, however, denied, and she herself was not above suspicion. The age of the mistress, 37, the slight nature of the nervous trouble from which she suffered, and the planfulness of the act were against the sleep-walking theory. The case was dismissed for lack of evidence, but the question remains, was it or was it not a sleep-walking crime?

V.—MISCELLANEOUS.

Psychologie de l'Attention. TH. RIBOT. Paris, 1889, pp. 180. F. Alcan.

The convenience of M. Ribot's compilations will make welcome all contributions from his pen. Like his former works, the present monograph opens out a comparatively novel topic and treats it suggestively. Attention is a concentration of the mind, a monoideism. The typical movement of thought is a polyideism; a lighting on one thought, dwelling upon it slightly, then flitting to another, and so on. Change, movement, is the normal law. Attention, furthermore, is of two kinds, spontaneous and voluntary. The latter is the one most commonly treated, but is theoretically less important. The more primitive is the former, in which normal vital interest guides the attention. We see this in animals attracted by all that is connected with food; in children staring at the brightest color and listening to the loudest sound. In voluntary attention we are guided by what experience has proved to be useful. We substitute an artificial end for the natural attractiveness, until the power of holding the attention becomes second nature. This is the power that is educationally so useful; the absence of which is typical in the shiftlessness, the instability of the hereditary criminal, and its presence typical in the devoted student absorbed beyond all interruptions in his chosen work. To the description and detailed exposition of each of these psychologic processes M. Ribot devotes a chapter. The third chapter is devoted to the morbid states of the attention. A variety of forms occur. There is the constant dwelling upon trifles, the fixed ideas, the reasoning mania, where one cannot get away from attending to one narrow trivial train of thought; there is the lack of concentrative power seen in idiocy, in the incoherence of mania, in dementia, and in milder forms, from the influence of drugs or a headache. Perhaps the apex of concentration is seen in the trance states of hypnotism, where the mind seems a blank, except the single point upon which the suggestion has been given. The religious ecstasies are of a similar kind, and the case of St. Theresa distinguishing seven stages of ecstasy is given as a type. The mechanism of attention is motor. It is initiative action. Whoever cannot control his muscles cannot control his attention. The motor accompaniments are not merely accessory; they form an essential part of the process. In concluding, M. Ribot completes his valuable survey by noticing the physical antecedents of good attention, regularity, a healthy nervous system, and the like. Brief as the monograph is, it offers about as complete and methodical a treatise on attention as we possess. J. J.

Recherches sur la fatigue mentale. FR. GALTON. Revue Scientifique, Jan. 26, 1889.

In this paper Mr. Galton gives a résumé of 116 replies sent in by members of an English teachers' organization to six questions of his on the signs and extent of mental fatigue. The answers bring together a considerable list of such signs, and furnish an excellent basis for subjective and objective observation. Of special interest is a case in which fatigue caused temporary fits of color-blindness. As tests for fatigue the following are suggested: the length of time that one can work carefully at a long piece of work; the promptness and certainty of memory for common things; arithmetical problems demanding "common sense"; the reaction time from hand to hand, which, of course, could only be employed with a class. Two conclusions are reached from the collation of the answers: one, that the reason mental fatigue is more wearing than physical is because it interferes with sleep; and second, that those most likely to overwork are those that work by themselves, especially those preparing for professions under unfavorable circumstances. The majority of students do not overwork; those that do are those that feel their power and are ambitious to succeed.

Erklärung der Sinnestäuchungen bei Gesunden und bei Kranken. Prof. J. I. Hoppe. Vierte Auflage. Würzburg, 1888, pp. 306.

This is largely a collection of cases of illusions and hallucinations from the psychiatric literature, largely supplemented by personal observation, with which the author seems to have busied himself very constantly. All states seem to have their appropriate hallucinations, though ordinarily we should not classify them as such. The illusions of sight naturally form the largest section of the work, and the contents of that may be cited as typical of the treatment. After distinguishing between hallucinations and illusions, the first section discusses the subjective phenomena of the retina; the second section gives the observations as drawn from what one sees when closing the eyes, and an account of the literature on this point. The third section gives explanations, and the fourth deals with illusions in particular. Hearing, touch, taste and smell are treated in a similar manner. The treatise is thus not an encyclopaedia of illusions, nor a well-developed theoretical exposition like the book of Sully, but rather a collection of cases and facts from which each one can elaborate his own views. J. J.

Consciousness of Lost Limbs. William James. Proc. Amer. Soc. Psychical Research, Vol. I, No. 3, Dec. 1887.

Circulars of enquiries were sent out and the answers compared. About three-fourths of the patients feel the limb after it is lost. Some had only a temporary and rapidly fading consciousness of the lost member. In one-third of the cases the toes may be moved at will. As faradization will restore the sensation of the lost limb, the loss of consciousness is due to habitual inattention. Since the real sensations of the limb if present are gone, hallucinations that may arise with reference to it are allowed free scope, and, like rudimentary organs, have also a tendency to vary. J. N.

The Psychology of Deception. Joseph Jastrow, Ph.D. Popular Sc. Monthly, Dec. 1888.

After an introductory exposition of the importance of the psychic addition to sensation in the process of perception, the author illustrates fully and explains three groups of false perceptions. The first is of the senses, a spoon seems bent in a glass of water; this is easily corrected by experience. The second is due to ignorance of the technical matters involved and of the lures that carry the attention this way and that; on this depend the tricks of conjurors. The third are those of expectant attention, mental contagion and the like, in which the chief deceiver is the observer's own self; here belong the cases of witchcraft and the cheap miracles of modern séances. For this kind of false perception the only help is prevention, the cultivation of sound and independent habits of mind, and a knowledge of when consciousness is likely to be an unreliable witness.

Statistische Untersuchungen über Träume und Schlaf. FRIEDRICH HEERWAGEN. Wundt's Philos. Studien, V, 2.

In order to answer the question, "Do people who dream much have lighter or sounder sleep than the average," a series of questions were asked of 142 women, 151 students, and 113 men of various occupations. Of these, 99 dream all night long, 133 often and 153 seldom, the remainder never; 216 have vivid and 175 not vivid dreams; 194 can easily recall the dreams, and 203 have difficulty in recalling them. 10.30 is the average time of retiring, and 0–3 hours are the extremes of the time needed to fall asleep. 261 sleep all night uninterruptedly and 143 have waking spells. 166 are heavy sleepers, 202 are light. 103 can sleep at will during the day (after-dinner naps not counted); 182 find the forenoon best for mental labor, 133 the evening, the balance are indifferent. 132 are of sanguine temperament, 70 are choleric, and 74 are phlegmatic; 20 are melancholic, the others are mixed.

These statistics serve to give an idea of the nature of the questions asked. The results were tabulated and the following laws deduced: Sleep is soundest in childhood, and becomes increasingly lighter with age. Dreaming reaches its maximum intensity and frequency at an age of 20–25 years, the increase from childhood and the decrease toward senescence being gradual.

The following factors are in close relation and vary together: Vividness of dreams, frequency of dreaming, lightness of sleep, power of recall, length of sleep or the number of hours needed, nervous or anti-phlegmatic temperament. Women dream more than men, and students stand as a class between other men and the women. Married women dream less than the unmarried. The deeper the sleep the easier it is to do mental work in the morning. A medium length of time required for falling asleep is connected with frequent dreams and light sleep. Men sleep most soundly and women least so. Those whose sleep is most interrupted also sleep most by day. Some who on rising feel weary rapidly regain vigor. There are, however, individual exceptions to all the above rules, as when frequent dreaming is connected with deep sleep; the stimulus that wakes a person up is provocation of a dream in this case. We must also remember that the curve of depth of sleep is lowest in the morning when dreaming is most frequent.

J. N.

Earthquake Effects, Emotional and Moral. JOHN MILNE. Trans. Seismological Soc. of Japan, Vol. XI.

Note on the Effects produced by Earthquakes upon the Lower Animals. JOHN MILNE. Trans. Seismological Soc. of Japan, Vol. XII.

In the first paper the author adds to a few general observations on the physical and moral effects of earthquakes, a number of extracts from miscellaneous sources, showing the state of mind into which men are thrown by such calamities, and finally traces some possible effects (but by no means all of them certain) upon national character. The most tangible are the creation of earthquake gods or monsters, and the introduction of their cults. In the second paper the question of animal foreknowledge of such phenomena is discussed. When the animal's agitation precedes the shock by some time it has nothing to do with it, but when by only a half minute or less the animal may feel the fainter ripples that run before the main waves in heavy earthquakes. Under favorable circumstances the shock may thus be anticipated as much as 10 or 15 seconds by human observers.

Vollständiges Lehrbuch der Gedächtniskunst. ADOLF KÜHNE. Osterwieck, Harz.

Die Gedächtniskunst im Dienste des Lehrenden und Lernenden. J. FIEWEGER. Breslau, 1888.

A. Kühne bases his art upon the laws of association and the principle that the learner should proceed from the known to the unknown. He approves what Kant called the *judiciöse* method in mnemonics, and maintains that without the help of the understanding the memory is always weak and untrustworthy. He adopts a figure alphabet, associates the fact to be remembered with the mnemonic word after the fashion of some of the older mnemonic teachers, and applies his principles chiefly to the learning of dates, tables and the like.

J. Fieweger gives many illustrations of the application of the older principles of the mnemonic art in learning difficult tasks. Such books are chiefly interesting psychologically as showing the cumbrous devices that some people find helpful for forcing the attention to disagreeable things. W. H. B.

Beiträge zur Kenntniss der Physiologie und Biologie der Protozoen. Bericht der naturforschenden Gesellschaft zu Freiburg, Vol. I, 1886.

This paper is of great psychological as well as of biological interest. It was found that when these unicellular organisms (*Stentor, Amoeba*, etc.) were artificially cut up, that the part cut off was restored to its perfect form by regeneration, no matter what was the portion of the cell that was lost; so that when the cell was divided two or more times into approximately equal parts, each part restored itself to the perfect animal. It was found that no portion would thus restore itself unless it possessed a fragment of the nucleus (which in Stentor is a long-headed filament). The conclusion is plain, viz. that any portion of the nucleus possesses protoplasm which has the complete characters of the entire being, and which controls the nutrition of the extra-nuclear protoplasm. That is, the molecules of the nucleus are *idioplasm*, and each is like the others, and in itself is capable of restoring the entire cell. These idioplasm molecules may of course be different from chemical molecules; experiments throw no light on that. Partial or incomplete division leads to the formation of individuals that remain united by the part left uncut. Studies of the spontaneous fission of these forms revealed that the daughter-cells of one form divided together, although separated into different watch-crystals and in different sorts of water. This same synchronism of reproductive activity of protoplasm closely related, was observed in the artificial sections, even when these differed greatly as to size. J. N.

NOTES.

The time of mental processes in the insane has been measured by Buccola, Tschisch and others, and recently again in Bechterew's laboratory by Marie Walitzkaja (*Wjestnik psichiatrii i nevropatologii*, 1888, VI). This lady worked upon seven insane subjects (3 general paralytics, 3 maniacs in various states of remission, one of which passed on into the excited stage while the experiments were in progress, and 1 paranoiac in a stage of remission), and for comparison, on five persons of sound mind, taking in all about 18,000 records. For the sane the figures are about normal; those for the insane show differences. In simply reacting to sound, the general paralytics alone were slow; most required more than 0.3 sec. and some nearly 0.5 against 0.168–0.207 sec. for the normal. In reacting to one of two sounds all the insane were slower, the slowest being those in general paralysis and in maniacal exaltation, those in remission approaching the normal. In reacting with one hand to one sound and with the other hand to the other, the delay was yet more marked; the normal time was 0.351–0.406 sec., that for those in general paralysis 0.707–0.943 sec., and that of a subject in maniacal exaltation 1.085. The association time was slower than normal for the general paralytics and for one of the others (one with a melancholic tendency), but quicker for the maniacal condition, 0.194–0.322 sec. against 0.664–0.716 for the normal.

Dr. Chas. A. Oliver some time since examined the eyes of twenty young adult male imbeciles (*Transactions Amer. Ophthal. Soc.* 1887) with a view to establishing what the normal eye should be when not used for near and careful work. He found them sound and as healthy in appearance as those of babies; and therefore concludes that such things as "insufficiency of the interni, dirty-red gray appearance of the optic disk, irregularity of physiological excavation, non-visibility of the superior and inferior portions of the scleral ring, absorbing conuses in all their varieties, increase in density and thickness of the retinal fibers, opacities of vascular lymph-sheaths, disturbed states of the choroid, and gross errors in astigmatism with changes in indices of refraction,"—things frequently found in used eyes, are the result of use and abuse.

Dr. C. Pelman, the director of the provincial institution for the insane at Grafenberg, near Düsseldorf, takes up, in a recent brochure (*Nervösität und Erziehung*, Bonn, 1888, 41 pp. 8vo), the question of educational hygiene. In assuming that nervousness is a distinguishing characteristic of the present generation, he offers no statistical proof, but appeals to the universal conviction. He understands by "nervousness," "conditions of a diseased excitability of the nervous system, which may also be well designated by the expression 'irritable weakness.'" Under the influence of a series of injuries, the nervous system loses its primitive power and stability. As a consequence, external impressions, which in a normal state would not be felt, or would be felt only in a moderate degree, excite abnormally great sensations which stand in no correct relations with the outer impressions. The draft thus made on the nervous resources is followed by greater weakness, and this weakness is nervousness. This form of weakness is the concomitant of increasing culture. Travellers agree in asserting that primitive peoples are seldom thus afflicted. While the American negroes were in the condition of slavery they were seldom nervous, but now mental diseases are common among them. America, according to Dr. Pelman, is "the promised land of nervousness." This is not, however, because of the superiority of American culture, but on account of the recklessness in the use and economy of nervous force which prevails there. It was in America that the worst forms of nervousness first appeared, especially neurasthenia. The cause of nervousness is found by our author in over-tension brought about by many particular circumstances, which may in general be summed up in "fast living," the prominent feature of city life, even in its accepted virtuous phases, and which may in turn be defined as continuous excitation without repose. The first and principal symptom in which nervousness expresses itself is the want of that power of resistance which every sound man is able to exercise against external encroachments. Constancy and firmness of character are lost. The man becomes too feeble to react against his environment, and becomes its slave. In extreme cases this loss of character extends to self-destruction, through inability to cope with the actual conditions of life. Less desperate cases seek the restoration of consciously lost power in the use of alcohol, opium and other drugs. Where restraining motives operate to preserve one from these, ideal equivalents are often sought. "To take in the hand a romance of Walter Scott's," says Dr. Pelman, "would be as tedious as comical. That is all so home-bred, so healthy, and if the end of the story is that they catch each other, wherefore so many pages and circumstances! How differently the modern romance-writers apprehend life! There one can see how it actually is, and a pistol-shot is a different solution from an ordinary marriage." After a brief discussion of heredity, in which he shows how nervousness leads to drunkenness, and the drunkenness of parents to the nervousness of children, the author touches the subject of education. "On education," he says, "devolves the double duty to make good the loss which birth has caused," or, in a word, to undo the evil of heredity. The author finds it impossible, as a physician, to approve of the modern education, yet he realizes the danger of merely one-sided criticism of a system which is, in its essence, so important to public welfare. He ventures to assert that "the child works *too early*, works *too much*, and works *badly*, that is, under unfavorable hygienic conditions." These criticisms are directed against the present requirements in Prussia, which are undoubtedly more severe than those in the United States, but would certainly apply to school exactions in many parts of our own country. The author pleads for moderation and diversity in the brain-work of boys and girls. He enlarges upon the evils of nervous over-strain for women. According to Candolle, the number of girls devoted to teaching, that have entered the Swiss asylums for the insane, is enormous. In England, the proportion of women teachers in similar institutions, according to Shaftesbury, in 1882, was astonishing, being 145 women to only 38 men. The proportion in Germany, in 1879, was not quite so great, being 186 women to 131 men. There entered in the course of the year 44 men and 56 women, showing a ratio of 7:10. Dr. Pelman adds: "What we have said concerning the large share which female teachers have in nervous diseases and weaknesses is true in a still higher degree of female office-clerks, bookkeepers, saleswomen, and post and telegraph assistants, and I am firmly convinced that there are few in these classes who do not bear through life their share of nervousness." As remedies the author names no medicaments, but urges a sacred regard for sleep, exercise, fresh air, the selection of sound partners in marriage, and avoidance of exciting competition. As a whole, the treatment is more suggestive of thought upon a wide-reaching and most important phenomenon in modern life, than satisfactory as an exact and specific solution of the problems which it raises. D. J. H.

Statistics of 1985 epileptic seizures in Dr. Féré's wards in the Bicêtre Hospital (*Compt. rend., Société de Biologie*, Nov. 17, 1888) show that nearly two-thirds of them fell between 8 P. M. and 8 A. M., and were grouped especially about 9 P. M. and 3-5 A. M. On the theory that dreams are most frequent in the first and last hours of sleep, a connection of the two suggests itself, and the questions are started whether both have a common cause, and whether the dreams influence the convulsions.

Bourneville and Sollier have contributed to the question of the proportion of physical abnormalities in idiots and epileptics the statistics of an examination of the genitals of 728 persons of these classes (apparently all males) in the Bicêtre Asylum (*Progr. Méd.* 1888, No. 7). Some sort of malformation of those organs was found in 262 persons, a number much in excess of the normal proportion. With epileptics whose attacks began after the first years of life, the proportion was much less than with simple idiots, and impotence was less frequent. On the other hand, varicocele was rarely present in the non-epileptic cases. Idiots that become such in very early life show greater malformations of all sorts than those whose defect dates from puberty or later. In so far as malformation results in impotence, it reduces hereditary disease of these kinds.

Of 10 persons caught feigning among 200 examined, Dr. J. Fritsch reports (*Jahrbücher f. Psychiatrie*, VIII, 1-2) only 2 as of completely sound neuro-psychic organization. Three more came short of actual alienation, but were neuropathic and excitable; five were fairly to be called insane. Seven were under arrest for crime against property, and two more had been. Criminals of this kind are fitted for feigning by their characteristic skill in lying, exaggeration and dissimulation.

A. Marro (*Annali di Freniatria*, I, 1888) has been led by observations on 22 paralytics to regard peptonuria as a characteristic accompaniment of progressive paralysis. In doubtful cases he would risk diagnosis on that symptom. It has been before observed in the insane, attending grave disturbances of nutrition, latent suppuration, etc.

In some diseased states of the nervous system, stimulation of a member on one side is felt in the corresponding member on the other. This phenomenon, called "allochiria" by Obersteiner and observed by him in tabetics and one hysteric, has been observed as a transient symptom by Dr. A. Huber (*Münch. med. Woch.* 1888, Nos. 34 and 35), in a case of multiple sclerosis of the brain and cord.

Five cases where spinal trouble gave a sharp boundary between sensitive and anaesthesic areas have been used by Prof. Eichhorst (*Zeitschr. f. klin. Medicin*, XIV, 5–6) for the discovery of the surface distribution of the nerves from the spinal roots. He found, contrary to common assumption, that the areas are not bounded by straight lines, but by curves, of which there were among his patients two types: one which showed on each side of the body an elevation between the vertebral and scapular lines, between the scapular and mammillary lines, and on the sternum; and another which showed elevations near the vertebral column, between the scapular and mammillary lines, and near the sternum, the last, however, not being united as before with the corresponding elevation on the other side.

An interesting case of what might be called motor musical aphasia has been added to one previously observed by Prof. Kast (XIII Wandervers. der Südwestdeutschen Neurologen und Irrenärzte, *Archiv f. Psychiatrie*, Bd. XX, H. 2). The earlier case was a talented member of a country singing society, who suffered right hemiplegia and motor aphasia from a wound of suicidal intent. The new one is that of a cultivated musical amateur (solo singer and violinist), a merchant of 45 years, who showed motor aphasia with disturbances of writing after a couple of apoplectic attacks. In both cases the perceptive musical faculty was preserved, and both recognized their failures in musical execution. The second lost his instrumental as well as his vocal music, though his ability to read notes was retained, his mental image of the tune was perfect, and the rhythm of his attempts precise. His speaking, which in the year and more since the attack had received the most practice, had returned much more completely than his musical powers, his whistling and singing being better than his violin playing.

Prof. Kirn, while admitting that there are criminals by inheritance, takes position (in v. Holtzendorff and v. Jagemann's *Handbuch des Gefängnisswesen*) against theories that would thus account for crime in general; there are those in penal institutions in whom psychology and anthropology can find no abnormalities. For the degenerate that are not fully irresponsible he approves imprisonment, but in institutions where they can be treated individually.

The *Journal of Mental Science* for January, 1889, contains a letter written to a well-known member of the Brit. Medico-Psychological Assoc. by a young woman who reformed after practicing laudanum drinking to the extent of four ounces daily. The letter is interesting in giving an inside view of the origin and mental and moral effects of the habit and of its cure.

Prof. Forel gives it as his experience (XIII Wandervers. der Südwestdeutschen Neurologen und Irrenärzte. *Archiv f. Psychiatrie*, Bd. XX, H. 2) that a short and complete stop in the use of liquor, even in delirium tremens, is harmless, from 4 to 5 days being generally sufficient for dishabituation. Nourishing food is essential, and should be given by force if necessary. Of 24 cases treated since September, 1886, at the insane asylum of Burghölzli, not a promising class of patients, 10 had so far (June, 1888) remained abstinent, 5 had relapsed, 2 were doubtful, 6 of unknown residence, and 1 chronically insane. Hypnotism was an important help with very suggestible subjects, but for permanent effect had to be united with entrance into a total-abstinence society.

The following circular has been addressed to medical microscopists:

"In behalf of 'The American Association for the Study and Cure of Inebriety,' the sum of one hundred dollars is offered by Dr. L. D. Mason, vice-president of the society, for the best original essay on 'The Pathological Lesions of Chronic Alcoholism Capable of Microscopic Demonstration.' The essay is to be accompanied by carefully prepared microscopic slides, which are to demonstrate clearly and satisfactorily the pathological conditions which the essay considers. Conclusions resulting from experiments on animals will be admissible. Accurate drawings or micro-photographs of the slides are desired. The essay, microscopic slides, drawings or micro-photographs are to be marked with a private motto or legend and sent to the chairman of the committee on or before October 1, 1890. The object of the essay will be to demonstrate: ***First***, Are there pathological lesions due to chronic alcoholism? ***Secondly***, Are these lesions peculiar or not to chronic alcoholism? The microscopic specimens should be accompanied by an authentic alcoholic history, and other complications, as syphilis, should be excluded. The successful author will be promptly notified of his success, and asked to read and demonstrate his essay personally or by proxy, at a regular or special meeting of the Medical Microscopical Society of Brooklyn. The essay will then be published in the ensuing number of *The Journal of Inebriety* (T. D. Crothers, Hartford, Conn.), as the prize essay, and then returned to the author for further publication or such use as he may desire. The following gentlemen have consented to act as a committee: Chairman, W. H. Bates, M. D., F. R. M. S. Lond., Eng. (President Med. Microscopical Soc., Brooklyn), 175 Remsen street, Brooklyn, N. Y.; John E. Weeks, M. D., 43 West 18th street, New York; Richmond Lennox, M. D., 164 Montague street, Brooklyn, N. Y.

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PARANOIA.

A STUDY OF THE EVOLUTION OF SYSTEMATIZED DELUSIONS OF GRANDEUR.

From the Clinical Records of the Bloomingdale Asylum, New York.

WILLIAM NOYES, M. D.

II.

In the number of this journal for May, 1888, the writer gave an account of the origin and development of systematized delusions of grandeur in Mr. G., at that time a patient in the Bloomingdale Asylum. He was under the immediate charge of the writer at that institution up to January, 1889, when he was transferred to the New York City Insane Asylum, on Ward's Island, where he now is.

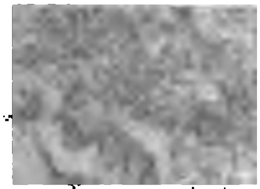
As there have been several interesting developments in his mental disease since then, and as it is possible to present further examples of his artistic skill, it is proposed in the present paper to continue the account there given, which extended up to about March, 1888.

Before beginning an account of his mental condition during the past year, it will be proper to add some facts regarding his early history that were not known at the time the first article was written. It is stated that as a child he showed some marked peculiarities of manner, being always fond of odd and fanciful articles of dress, and this was especially the case with regard to his hats and caps, he being fond of decorating these, or of wearing something that was different from the other boys. It was always hard for him to concentrate his mind on his books, and although learning readily, he was lacking in steady application.

One or two further peculiarities of conduct during his life at Bloomingdale have been brought to notice by his attendant. His practice after each meal was to go out to smoke, remaining out often half an hour or more. On his return to the hall he had one method of procedure, from which he never varied. He first washed his hands in the bath-room; then going to the dining room, he filled a glass with water from the cooler, and holding this extended in his right hand, he would balance himself on one heel and suddenly whirl about, always to the right, and would then drink the water. The force of his turning was often sufficient to throw some of the water out of the glass.

During the summer a New York artist, who had known Mr. G. in Paris, called on him at the asylum, and through this gentleman some particulars were learned about the patient's art-life abroad. As would naturally be expected, the same characteristics were noticed by his fellow-students in Paris that have been already referred to.

He was always looked on as exceedingly bright and clever, but entirely lacking in application and in the



ability to finish his work; in fact he had become known as "the unfinished artist," a particularly apt characterization. The water-color sketches which the patient showed to this gentleman during the call were said by him privately to be of the same general character as the patient's earlier work, strong and original in conception, but lacking in refinement and delicacy of finish.

Twelve of these water-color sketches are here reproduced, some having been made during the past year, and others earlier. The odd and fantastic conceptions are sufficiently evident to need no special comment. The first one, in the upper left-hand corner, he has called PUCK'S PLEASANTRIES, then comes a FRENCH COOK and UNDINE, and last an IMP AND FROGS. This last one and the next in order, THE WEST WIND, were done previous to 1888, the remainder in 1888. The remaining three are THE DANCING FROG, a second COOK, and PLEASURE, TIME AND YOUTH. The Dancing Frog he presented to a fellow-patient, together with the following original verses:

"Hay dai! Hay dai! for the month of May,
With its flowers and flocks all blithe and gay:
When the scent of the meadow and smell of the pine,
For a perfect perfume in a union combine;
And the lambs and the lassies all skip to the lea,
To dance in the clover and sport with the bee.
'Tis a season of song and a season of cheer,
So tune up the fiddle and tap the fresh beer;
And trip to your lassies as long as ye may,
For youth is for pleasure, so let us be gay."

The coloring in the last sketch, PLEASURE, TIME AND YOUTH, is particularly good, and the whole makes one of the best things that he has done. It will be seen that it is allegorical, as is most of his work. Pleasure, in the shape of a woman, is leading on the youth, who holds a wine-glass in his left hand and a bottle in his right, while behind stalks Time, endeavoring to attract Youth's attention to the fast-flowing sands in the hourglass.

The legend attached to this is a good example of one of Mr. G.'s numerous puns; it reads, *Life's fitful Fille-vers.* The *Fille* refers of course to the maiden in the sketch, and if the *fille* is spoken rapidly with the French pronunciation, we get *Life's fitful Fevers!*

Since the appearance of the first article on Mr. G. the writer has often been asked if, with the marked talent shown by the patient in those sketches, and which is equally well shown in these, it would not be possible to induce him to do some regular work. All attempts, however, to bring this about have been without result. The artist referred to above told him that he could furnish him with work if he would only make the attempt to do it, but the patient's reply, as on all former occasions when the subject was brought up, was that "The spirit does not move me." Being passionately fond of the theatre, he has often been offered a theatre-ticket if he would only make some small sketch in return, but he prefers to lose the enjoyment of an evening's pleasure rather than to try to force "the spirit." Only once did he ever make a sketch under compulsion, as it were, when he made for the writer, in exchange for a theatre-ticket, the beautiful water-color sketch that serves as a frontispiece for this article, and which he called PARADISE AND THE PERI, his conception being taken from Moore's well-known poem, the sketch representing the first two lines:

> "One morn a Peri, at the gate
> Of Eden stood, disconsolate."

Through the kindness of his family and of one of his friends it is possible to present here representations

of three of his early works, begun and finished before his mental disease developed.

The two upper ones in the group of three are water colors, both harmonious in design and execution, the larger one being especially pleasing. The lower one is a landscape in oil, and unfortunately is not a subject that can well be represented by a photograph, but the original is a very meritorious piece of work, and has been highly spoken of by one of the first landscape painters in America.

How great a change has been brought about by his mental disease can in no way be better appreciated than by comparing the quiet and restrained beauty of these works with the group of five water-color sketches at the end of the article. In the center is the hideous witch. In the upper left-hand corner Puck is disporting himself with a frog on a toadstool, while opposite is Sycorax, the mother of Caliban. In the lower right-hand corner is another Witch, and on the other The Vampire looks out from her cave of darkness, a human skull lying before her, and blood dripping over the edge of the rock.

Mr. G.'s chief occupation during the summer and autumn of 1888 was the writing of a book of 200 quarto pages, with the following title-page:

THREADS OF THOUGHT.
Gleanings gathered from the Greek Gods.
Siftings from the Sands of the Sighing Sea.
From Beer-sheba to Dan and on.
From the Dead Sea to Daylight.
Alpha and Omega.
Fret not thy Guise-ard.

"The mills of the gods grind slowly, but they grind exceeding fine."

"Her 'prentice hand she tried on man, and then she made the lassies, O."

"Whom the gods wish to destroy they first make mad."

Opposite the title-page is a pen-and-ink sketch serving as a frontispiece, representing the GODDESS OF MUSIC sitting on the crescent moon, playing on the harp. The legend attached, which at first sight appears to be an inscription in French, is in reality a pun on the musical scale, *do, re, me, fa, sol, la, se, do.*

He divided the book into twelve parts, one for each month in the year, and at the beginning of each month is given the mythological sign of the zodiac, the member of the body for that month, the tribe of Israel to which the month is dedicated, the name *Christ Jesus*, one of the Apostles, one of the Holy Stones, one of the days of the week, and finally, the name of one of the ancient churches. Following this is a description of the mythological person to whom the month is sacred, after which comes a description of some Greek or Roman divinity, from some dictionary of antiquities. On the page opposite to this description is inserted before each month a pen-and-ink drawing of the mythological hero or heroine that the ancients associated with the month.

Opposite January is Aquarius, and the arrangement of the descriptive matter for this month will serve as a general example of the arrangement of all the other months:

"Jove. Zodiac. Aquarius. The Waterman.
A. Member. Legs. Les jambes. Le gambe.
Month. January. In honor of the god Janus.
Tribe. Simeon. Hearing with acceptance.
Christ Jesus.
Apostle Matthew. Gift of Jehovah.
Gem. Jasper.
Day. Monday, sacred to the moon.
M. Church. Ephesus.

NEPTVNE
JASON
POLLVX
Sub Rosa

Y. Janus. Represented with two heads, and called therefrom Biceps—the keeper of all gates—also called Matutinus, as opener of the day; represented with a key in his left hand and a staff in his right. He was also called Quirinus because presiding over war. Clusius and Patulcius as 'shutter' and 'opener' of gates. He was called Curiatius because he was charged with the care of the world. Janus and Jana the *solar* and lunar deities, derived from Oriental names of Divinity, Jah, Jao, Jovis, whence Jom or Yum, 'the day.' Quirinus is derived from the Sabine word curis, 'a spear.' When represented four-headed he was denominated Quadrifraris, and is identical in appearance with the Brahma of India. He was like Hermes, the guide of souls from Purgatory to Heaven."

The illustrations preceding the twelve months are as follows: Aquarius opposite January; Neptune, February; Jason, March; Taurus, April; Pollux, May; Infant Hercules, June; Achilles (with the spear), July; Pandora, August; Libra, September; Aegyptus, October; Apollo (drawing the bow), November; Bacchante before Pan, December.

Aegyptus is represented by two drawings, the seated Egyptian figure and the sacred bull. A long description is given of each of these personages, the circumstances of their birth and parentage being given, together with their adventures, and the rites with which the divinities were worshipped.

After these mythological characters have been described, Mr. G. has set down in each of the twelve divisions of the book his own thoughts on different matters, mostly religious, and quotations from the Bible, short poems and extracts from poems. Proverbs, old saws and original verses are mingled freely together without any order or coherence. His remarks on religious subjects are often strikingly forcible and original, and sometimes remind one of the impassioned utterances of a revivalist preacher, and they are not seldom poetic in beauty and expression.

The description of Janus already given is immediately followed by these quotations:

"Fortitude is better than biceps for building.
'He who tholes conquers.' Allen Ramsay.
'It's good to be sib to siller, but more siller
When you're sib to '*The King*.'"

And these are followed by a song from "Love's Labour's Lost," after which come various remarks on religious subjects:

"Man that is of woman born is of few days and full of sorrows. He learns to lisp words the meaning of which he knoweth not. He learns the alphabet and how to form words. But all this learning to him makes him like to the pig fed upon the pod of the pea; a higher intelligence first eats the fruit of the soil and then, having fed him upon the husk, eats him. And so the greater 'Good' absorbs and refines the lesser lights, so a penny dip may eventually become a particle of the great and glorious 'orb of day.' In the beginning was the Word, and the Word was with God, and the Word was God. The same was in the beginning with God.—St. Luke, 1st c., 1 and 2 v.

"The infant at its birth is simply the jar (Jah) in which is planted the holy seed, or germ of the Word, which is to grow and flourish, according to the love and devotion bestowed upon it by its parents. Until the child is able to express a thought of its own, it is no more than the journal in the which you write the occurrences of your daily life, and which must be opened and like a flower allowed to bloom in order to find color and expression. Few people appreciate the mighty magic of the Word, or realize that the thoughts we think (which are nothing more than the combinations of the Word of God) generate either manna or rank poison in the hidden tissues of the body, and that we transmit this good or evil in a greater or less degree to whatever we come in contact with, through limb or lung. Like seeks like all the world over. Consequently, if you are harboring evil in your heart you will not only garner what you gather, but the evils of your neighbor will fly to you, if your power of evil outweighs his; just as the needle is attracted by the stronger magnet.

"Man is the only creature to whom is allotted the power of selection or choice; the dog must always be a dog, until evolution passes his spirit into some higher form, but man may have the semblance of the god, and the heart and habits of the beast, all through his own choice and a failure to honor the commands of God. The body of the beast cannot hold that which is Godlike; it would burn it up, for our God (the God of Love) is a consuming fire. If we dishonor the temple of God, our mortal bodies, by gathering wrath and holding it, we shall be driven out into the abode of some beast (as was Nebuchadnezzar) until such time as our sin shall have been purged away. The teachings of Pythagoras were just as much inspired by God as those of a broader doctrine delivered by Paul. But Pythagoras drove the spirits of erring mortals back into the beast and then taught us hope of redemption, while the teachings of

Christ constantly raise the soul of man from the groveling worm to the heights of heaven. Forego wrath, let go displeasure. Vengeance is mine, I will repay, saith the Lord. Wrath and anger are but the outgrowth of a longing for revenge; they are alchemists concocting vengeance in the secret chambers of the heart and liver. Turn them out, let them go, they are thieves and robbers. Better a dinner of herbs where love is, than a stalled ox and hatred therewith. If evil thoughts come into your heart don't give them house-room, purge them out, and above all give them no utterance, for if you do it is your endorsement to a note of hand signed by your enemy, and ten to one a note he will leave you to pay. Never call another a liar; make them, if you can, put their statements in black and white over their own signature, then they will have certified to their own lie; let you add a little of the grace of God thereto, and a very good purge and cleanser may be evolved therefrom, and society thereby relieved of much filth and ugliness.

"The man who keeps cool
Needs no compass or rule,
For his measure or guide, when he builds;
For his God in the sky,
Has him close to his eye,
And turns back the foe, when He wills.

* * * * * * * *

"The name you bear is one of God's names; you received it through baptism, at the hand of God's servant, one of his ministers; in like manner so did your parents before you. It is your bounden duty as a Christian to learn to know the full meaning of this God-given name, and then to live up to it. If we learn to listen to the quiet spirit that ever dwells *within* the house (the one that sings in our heart when we are gay and glad and thankful) and follow its teachings, we shall ever find the Temple of God filled with His presence, the Shechinah[1] that veiled the mercy-seat of old; and the quick spirit of discord, so prone to argument and discussion, who is an eavesdropper and a listener at corners, and who is ever ready to fly upon the tip of our tongue and hail a storm of words, he will fly away to some more congenial habitation.

"Merlin built for King Arthur a royal habitation, filled with all the luxury and elegance that wealth or art could furnish or devise; and on the castle's peak, spreading its wings to heaven's blue expanse, he placed an angel of burnished gold, symbol of the peace and love that should reign within the palace walls. To this edifice of beauty thronged the knights of King Arthur's Round Table, to joust and tourney; and merry feast and woman's smiles were not sufficient to keep them contented and happy. The devil of unrest, envy, and worldly ambition entered their hearts and showered upon them the seeds of lust to suffocation. They parted, and sought in devious ways to find the Holy Grail, not knowing in that dark and stormy day that the Kingdom of God is in man's heart, and that His hidden treasures are always to be found there. The warriors of the Table Round one by one crumbled into the dust from whence they came, the golden angel raised its pinions and soared from the palace peak; grim Time at last swept away all traces of a king's abode, and nought remains to us but a legend of knights who came to grief and the grave through lack of honor and lechery. Mark you, my friend, your name, and your body that bears it, comprise your palace of Truth, above which God has placed the guardian Angel of Peace and Love; see that you wander not from 'Home'; seek not the laughter of strangers; be content with the arrows in your quiver, or you, too, may meet with the same disastrous end as the knights of the Table Round.

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[1] See Mr. G.'s water-color sketch of SHECHINAH, in the first article, Am. Journal of Psych. I, 475, and page 16 of the reprints.

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"Water contains just the same subtle qualities to-day as it did when Christ changed the water into wine at the marriage of Cana. But we should be careful how we use it, for if you mix with it other than good thoughts and thankfulness, it will produce no wine in your jar, but, on the contrary, something very much resembling poison in its action. It is not what we eat and drink that hurts us, but what we mix with it from our own internal infernal economy.

"'Spare the rod and spoil the child' is not to be taken literally, for the teaching to be conveyed lies deeper than the surface of the line. The child is the recipient of a reasoning faculty which, if you cultivate by argument and teaching, will spoil the child, in one sense, by developing that which is manly in its nature. We beat an animal because it has no power of argument or speech, but even in this case moral force is better than blows; for the spirit that animates the brute and causes it to live is the same that pulsates in the heart of the babe, and may be reached in the same way, although we hear no audible response to our admonitions. Blows to brute or babe are always an evidence of lack of patience and want of wit. Striking a child for a misdemeanor is as sensible as beating your own head against the wall for some error committed; you blunt the very faculties you wish to build up and improve when you use brute force instead of reasoning and logic. Furthermore, you alienate the nature that has been given you to love and cherish; you repel it instead of drawing it towards the light and warmth of your own intelligence; you not only extinguish its fire, but you are destroying your own as well, for the child is given us that he may replenish the holy fire upon our hearthstones as it burns low.

"Brutality drives the angel from the door and places a fiend in its stead. Brutality is a boomerang that recedes upon him who uses it as a weapon with redoubled force. Wisdom's ways are ways of pleasantness, and all her paths are peace.

"Prayer to God is always answered; we are not always aware of the fact; when the answer comes to us we fail to recognize it because we have made ourselves spiritually blind. We cultivate our spiritual nature to such a degree that the soul becomes like a diamond in a bladder; its beauties are hidden because it is shut out from the light of that great power called God that made it and its wonders. Man, in the course of his life, is constantly brushing past the Angel without recognition, simply because he is ever plowing the fecund earth with his nose, like any mole. Look neither up nor down, but straight ahead, fair into the eyes of your fellow-creatures; God's universe is there and His kingdom infinite; His messengers respond quicker than thought; His mandates pass swifter than the flight of bird. His eyes are everywhere beholding the evil and the good. 'The wish is father to the thought'; therefore be careful that your thoughts assume not the garb of the Destroyer, for you must e'en play the part if you don the robe; you must fill your role or quit the scene."

Here follows a page from *The Light of Asia* and several old sayings, after which is the following on Good Thoughts:

"Good thoughts breed other good thoughts; as the snowball rolls it gathers its like and grows. There is no gift so gladdening as fair speech; other bounties that we lavish upon each other fade away and are lost and forgotten sometimes, but the music of kind words comes ever echoing through the deep defiles of the past with all the freshness and joy of the day that ushered in their birth. Oh, blessed God-gift of speech!

"The Bible is Life's great mentor, to which we should go for confirmation of our beliefs and acts; it is the standard by which man is to acquire the art of right thinking and right doing. '*Ars longa, vita brevis.*' The Old Testament is a history of the natural man living under the influence of a God whose personality is undefinable and beyond his comprehension; whose presence is only realized through his manifestations in Nature. The New Testament is the development of a new birth in this natural product of the earth and its forces, in which the mind of man is ushered into a new light and made to realize that God walks in the hearts of men and in time transforms them into a perfect image of himself. The natural man resists this new birth just as the elders of the church resisted Christ and finally crucified Him; the earthly and spiritual natures cannot exist together—the one crowds the other out—for one is a destroyer, a consumer, while the other is always a builder. The spiritual nature in man never feeds upon itself, as the physical nature often does, but is constantly conceiving new combinations, evolving new thoughts in the great storehouse of the mind, and sending them out on errands of love and mercy. The spiritual part of man is like the widow's cruse; draw from it as you will, it never empties, for it is a portion of God, and consequently inexhaustible."

The above is all from what he had gathered together under January, there being rather more religious matter than in the following months.

February contains much less that is of interest, the first part of the month being taken up with quotations from the Bible, Shakespeare, and a long account of Neptune. Later on we find the following:

"He who goes through life with his nose constantly in the ground may become intimately acquainted with the worth and ways of bugs and sods; but he will miss the great glories of the universe and the mysteries of the brotherhood of man. Head erect, eyes to the fore, and remember God walks in the secrets of the heart.

* * * * * * * *

"All the Greek and Roman deities are but different phases of that Great Power which we now worship as God. And these various forms of worship were given to man that the spiritual child might climb to the realization of the Infinite by easy grades, and not be startled into frenzy, madness and death by the glory and magnitude of Him who guides the universe. The ancients were great and successful in proportion to their faith in their gods and the integrity of their devotion. 'Just as men to-day make a god of money and bow to the golden calf, are the most successful in gathering money, who are the most sincerely devoted to their idol. But all the gold in creation will not save a man from the hands of death, or buy back his soul when once he has pledged it to an idol of gold. Gold is a good means to an end, but should never be mistaken and worshipped as the end-all and be-all of life. All the men in history from time immemorial who have tried to lift themselves into power and thereby gain happiness by a golden lever have been signal failures.

"The force of love is so great, so strong, so intense, that if a man were to fill his store-houses with it at one fell swoop, before preparation had been made for such commodity, it would burn him and all his belongings up. *His* ministers are a flame of fire.

"Richelieu said to Baridas, 'Behind thee stalks the headsman.' The same might be said, with equal truth, to each one of us: for each man bears his judge with him, who condemns or justifies as the case may be. The thief, the assassin and murderer always fly the hand of justice, but they never by any chance evade their doom; they may miss the hand of man, but the hand of justice presses them closer than their own shadow. Often through the retina of the eye does conscience throw upon the ambient air the figure of the murderer's victim. A man can no more shirk the responsibility of his own misdeeds than he can shake off a fever by changing his coat."

March is devoted almost exclusively to mythology, and need not detain us. In April he has given his idea of *evolution*, and its failings as applied to man.

"The respects in which evolution as a necessary process in the natural and brute worlds does not wholly apply to man:

1. Instinct yields to conscious intelligence.
2. The struggle for existence yields to a moral law of preservation, and so is reversed.
3. Intelligence takes the place of natural selection.
4. The will comes into supremacy, and so there is a complete person. Man, instead of being wholly under force, becomes himself a force.
5. Man attains full, reflective consciousness.
6. Conscience takes the place of desire.
7. The rudimentary and instinctive virtues of the brutes become moral under will and conscience.

8. Man comes into consciousness of God.

9. Man's history is in freedom.

10. Man recognizes and realizes the spirit.

"Contrasting phenomena of evolution under necessity and evolution under freedom:

1. Man changes and tends to create his environment; achieves it largely, and so may prolong it. The brute adapted itself to environment, but had no power over it.

2. Man progresses under freedom. The brute progressed under laws and environment; man, under will and moral principles of action.

3. Man thinks reflectively, systematizes knowledge and reasons upon it; the brute does not, except in a rudimentary way.

4. Man has dominion; the brute is a subject.

5. Man worships, having become conscious of the Infinite One; the brute does not.

6. Man is the end of creation, and the final object of it; the brute is a step in the process."

In the first article a description was given of two of his religious diagrams, representing *The Sealing of the Holy Spirit*, and this he refers to in the following description of the wheat-berry; elsewhere in his book he quotes from a magazine article on the form and structure of the wheat-berry:

"The wheat-berry has seven coverings, the same as man has the seven seals of God upon his spiritual grain or soul. Sometimes the worm (called envy, hatred, and malice) perforates these coats in the first Adam, and the spirit writhes out from its temporary abode, in agony, like the wrigglings of matter strained through a colander. This is the agony of death, but when the seals are kept intact until the season of perfect ripeness arrives, the transition is one of perfect peace, and consequently painless. Man, in spite of the laws of nature and all that has been written or said regarding the theory of evolution, would fain skip the chrysalis stage and jump from grub to butterfly, without any prolonged period of durance-vile. Patience is a hard lesson to learn, no matter how you fix it, for man is born with the seed of the tyrant lurking somewhere, and wants to rule."

At this point is inserted the drawing of the FISHERMAN AND THE GENII, which is to illustrate a story in the Arabian Nights. It is the one under which is the inscription

"Vir . . . . man
Gin . . . . trap,"

this being another pun, the Latin *vir* meaning *man*, and *gin* a *trap*, while combined, *virgin* equals *man-trap*.

There are some original verses descriptive of this drawing, called *Nettles, etc.*, describing the terror of the fisherman on opening the old earthen coffer, to be confronted with the air-spirit.

This concludes the month of April. In the month of May, besides the usual mythological stories and Bible quotations, is the following original poem:

"Oh! Peace beyond the power of thought;
Oh! Happiness by good deeds wrought;
Oh! Sun that never quits the day;
Oh! Lillie pure as chrystal spray:

"Oh! Living mandate of our God;
Oh! Pathway that the Saints have trod;
Oh! Zephyr from the perfect blue;
Oh! Lambkin born of stainless Ewe:

"Into our chamber's holiest ground,
Find entrance meet. Let love abound.
Oh! fill Thou both our bed and board
With the full spirit of the Lord.

"Guard well the lintel and the sill,
'Gainst lurking foe and hidden ill,
And let no discords enter there,
Nor thoughts that work the soul despair.

"But firm in faith, secure in Thee,
Let worship of the Holy Three
Purge from our hearts all evils done,
And bless us with 'Thy Kingdom come.'"

This is followed by other original verses, and in these he makes the only known reference to the fact that he is confined in an asylum:

"1. Immured in a mansion made for madness,
Where naught that doth pertain to gladness,
Enters the gate or through the door,

"2. I count the hours long and dreary,
And ruminate and grow most weary:
The slack of time is such a bore.

"3. I think, perhaps if I played crazy,
This life which now seems dull and hazy,
Would change, if I my clothing tore.

"4. A jig, or fit, some caper funny,
Might add to life a little honey,
And in the heart some balsam pour.

"5. But here to sit like wart or bunion,
Or bulbous growth upon an onion,
And know the gay world never more—

"6. I'd rather rise by slow cremation,
And leave this dismal dull damnation,
By giving up this mortal corps.

"7. What! eat and sleep and then grow musty,
While all your senses clogged and rusty,
Refuse to act as heretofore?

"8. I'd rather hang upon a gibbet,
And make old bones for flies to fidget,
Than sit here till my sides were sore."

May seems particularly to have inspired him poetically, for after these verses come two more series, and he concludes the month by quoting the exquisite little poem by Austin Dobson, *A Fancy from Fontenelle,*[1] which he has illustrated by the Maiden and the Gardener (directly under Taurus).

The succeeding picture of the series, entitled FAITH, also has an original poem given to it, but it is rather mystical and obscure. The two cherubs are intended to represent the *Gemini* of the zodiac, in some spiritual sense, in the same way that Castor and Pollux represent it physically.

June is ushered in by the picture of the INFANT HERCULES (following FAITH), and the month is mostly taken up with a description of the adventures and labors of Hercules, and is concluded by the picture of the spirits blowing on THE CRAB, the crab being the zodiacal sign for June.

ACHILLES with his spear faces July, and in this month the artist has placed the graceful little figure LA SONNETTE, Cupid ringing the bell to awaken the maiden within. This is accompanied by some verses

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[1] As illustrative of Mr. G.'s refined sense and appreciation of beauty, perhaps it may not be out of place to quote the verses here, as they were intended to accompany the drawing.

A FANCY FROM FONTENELLE.

"De mémoires de Roses: on n'a point
vu mourir le Jardinier."

The Rose in the garden slipped her bud,
And she laughed in the pride of her youthful blood;
As she thought of the Gardener standing by—
"He is old—so old! and he soon will die!"

The full Rose waxed in the warm June air,
And she spread, and spread till her heart lay bare,
And she laughed once more as she heard his tread—
"He is older now. He will soon be dead."

But the breeze of the morning blew, and found
That the leaves of the blown Rose strewed the ground,
And he came at noon, that Gardener old,
And he raked them softly under the mould.

And I wove the thing to a random rhyme,
For the Rose is Beauty; the Gardener, Time.
Austin Dobson.

IBESAMI
MINERVA

SESAME
PANDORA.
LIBRA.
MINERVA.
HERMES.
BACCHUS
AMOR
APOLLO.
CAPRICORNUS
AMO
BEAUPRET
LE VOILÀ AV VENT.

which are somewhat more graceful than many he writes :

"LA SONNETTE.

"When Love comes ringing at the bell,
And finds no maid to heed his spell,
He simply sighs and lifts his eyes,
And hies away with Alack-a-day!

"When Cupid comes on dewy wing,
What are the notes the zephyrs sing?
Come laugh and sing and greet our king,
For while we may let's make our hay.

"What says the swaying of the bell,
What are the words its tinklings tell?
Come to the lea, 'tis he, 'tis he!
The merry monarch blithe and free,
Leave bin and care and hail the heir.

"What says the Sun, when day is done?
The maids were many, the bride was one.
Who loves him little should love him long.
So rhymes the riddle to suit the song,
Come robe the king, and favors bring."

The two pictures following, SUB ROSA and the MAIDEN AND CUPID, are inserted without any verses or explanatory notes.

The drawing SESAME is more deeply allegorical than many of the others. On the right-hand side of it he has written the names of the seven coverings of the endosperm or center of the wheat-berry, and below is the verse from the third chapter of Genesis :

"So he drove out the man ; and he placed at the east of the garden of Eden, cherubim, and a flaming sword, which turned every way, to keep the way of the tree of life."

And on the opposite page, verses from the fifth chapter of Revelation, beginning, "And I saw on the right hand of him that sat on the throne a book, written within and on the back side, sealed with seven seals. And I saw a strong angel proclaiming with a loud voice, Who is worthy to open the book, and to loose the seals thereof? . . . . And in the midst of the elders stood a Lamb as it had been slain, having seven horns and seven eyes, which are the seven Spirits of God sent forth into all the earth."

He thus appears to connect the angel that guarded the gate of the Garden of Eden and the angel before the throne; and it is especially interesting to see the influence that the number *seven* has on his mind, as was the case with the Seven Sealings of the Holy Spirit in his religious diagrams.

Next in order comes the sketch of PANDORA, with a description of her attributes, and then several pages of description of the beliefs of the Buddhists, on which he comments:

"All these theories and beliefs relative to the great Creator are but so many pieces taken from the great mosaic of the Bible. All the existing religions in the world are simply stars in God's spiritual pyramid, the apex and key-stone of which is Christ. The Holy Bible is the point from which all beliefs radiate; it contains all that man knows or may know of that Supreme Being we call God. All the secrets that perished in the Alexandrian Library are hid between its two covers, and may be found by those who tread the path of love; but let no one with hate imbued start upon the quest, for the Holy Book is the Gate of the Garden of Eden, and He placed of old at this gate an angel with a sword of flame, to guard the ground from guilt and shame."

Reference was made in the former article to the fact that he considered himself as undergoing a probationary stage, and would in time become worthy to receive a great spiritual revelation. This is made more clear by the following:

"When the realization of the Christ first breaks in upon man's nature, it is the dawning of his spiritual day, the first gleam of the soul's sun. 'The Sun of Righteousness,' from whence we derive light and life, and around which spiritual point revolve all earthly changes until this Sun is realized in its perfect magnitude, filling space, and being one and inseparable with God. When once this truth has germinated in the heart of man, if only one thin ray of the heavenly light has penetrated to the secret recesses of his nature, its influence never leaves him; it may be obscured by the fogs and mists of doubt and clouds of earthly cares and vexations, but high above the vapors of the world rides this Great Redeemer, who is constantly drawing us imperceptibly to himself, while the fire of His love is purging and cleansing away that which is mean and perishable in our being, and fitting us to behold the full glory of His majesty, and to stand in His presence without fear and without reproach. Man cannot gaze on the physical sun without blinking, much less could stand the glory of the spiritual sun; the vision would consume him, unless he had passed through the incarnations which give one the requisite spiritual strength. God's mercies are manifold, His mysteries marvellous and unanswerable, and that which He hath begun abideth forever. Allah illa Allah!"

The sketch of LIBRA faces September, and MINERVA follows, with a description of the goddess; and the remainder of the month is mainly given up to religious subjects.

September is prefaced by AEGYPTUS, the zodiacal sign of the month, the scorpion, being seen in the circle above the seated Egyptian figure, while *Aegyptus* *the Bull* follows, with an account of his worship. He comments on the Egyptian priesthood, and incidentally on *lunacy*, as follows:

"The confusion of faces, spoken of in Daniel, is the same physical phenomena that gave to the Egyptian priesthood, and that of some of the other nationalities, gods and goddesses with the heads of birds and beasts. When people worship the creature more than the creator, then comes the messenger of the gods, Mercury, and takes possession of the human body, and throws upon its surface whatever he finds reflected upon the mirror of the mind; hence diversity of aspect and consequent confusion. There is nothing hidden but shall some day see the light. This phenomenon is to-day stigmatized as 'Lunacy,' and the cunning and science of medicine generally succeeds in driving out the angel and sustaining the devil or beast, which are one and the same thing. Spiritualistic mediums are under the same influence, and usually end their days in some insane asylum, unless they change their mode of life, for they are as a rule lecherous scatter-brains, either in thought or actual practice. This is a rod by which the great good God drives men to prayer and search of Him, the Creator of all things."

November being represented by Sagittarius, The Archer, is appropriately introduced by APOLLO, shown in the act of drawing the bow, and a long description of him follows; and then comes BACCHUS and his story, and Barry Cornwall's poem *To Bacchus*. A second sketch of APOLLO, here more appropriately shown with his lyre, is followed by a poem *To Apollo*, by L. N. Torre.

December is introduced by the DANCING BACCHANTE, instead of by Capricornus, the proper zodiacal symbol, this latter picture being placed in the middle of the month; and the story of Pan is told. A rather good comparison is here given when he says:

"Man is the social chameleon, whose mind, morals, moods, and manners are colored by the thoughts on which he dwells."

The remainder of the book is taken up with religious reflections, and these quotations may not inappropriately be closed by the following, which might not seem out of place in the writings of a more ambitious and fortunate religious teacher:

"Men complain of a continuance of bad luck because they have not prepared themselves in a way to deserve better. People cry out against Nature and Fortune because their crops fail and their children turn out bad, when the fault lies at home. Hell is just as hot and horrible as we have a mind to make it, and heaven creeps into a man's heart when the place is fit for its indwelling; it is a condition, not a locality limited by any bounds save such as are spiritual and consequently limitless. You don't have to reach to heaven or fly to it; it comes to you when you have finally won it, and it makes you a part and parcel of itself; in other words, it is the spirit of purity, the spirit of Christ dwelling in us and making man his own. Growling and grumbling at the humble trivialities of life often shuts the angel from the door—turns away God's messenger, and retards the dawn of that bright day of more perfect light, when we shall no longer see as through a glass darkly, but face to face with truth revealed in all its native loveliness."

The sketch Le Voile au Vent and the graceful and beautiful Cupid at the door complete the illustrations.

The mottoes on this last sketch are "Nemo me impune lacessit," which he translates, "No one wounds me with impunity," and gives it as a motto of the Scots; at the right-hand side "Beauty conquers all," representing the "Candore omnia vincit," on the left; and "Truth is mighty and will prevail."

The written matter is brought to a close by the following page:

"He laughs best who laughs last.

"T, he, some of the means equals t, he, some of the extremes.

"These are the aberrations of a fitful and fit mind.

"Let none erase, nor pen, or trace,
  Upon this book of mine;
In solitude I've writ it all,
  And thought upon each line;
There's much within that is my own,
  And much that is divine.
So let no sacrilegious cur
  Pollute the page
With blot or blur."

The three remaining illustrations at the end of the page would have found a place more appropriately in the first article, but it was not possible to give them at that time. The larger one represents a photograph of his bureau drawer, which well illustrates his passion for order and symmetry and for the regular and harmonious arrangements of lines. The order of the arrangement of these articles never varied from day to day, and was maintained with scrupulous exactness. The carved bowl, with the motto *Pro Rege in Tyrannos*, has already been described. Above this is what he called his *Chestnut*, and represents a shallow lead receptacle with a cover fitting over it, in general shape resembling a chestnut. It may be recalled that the lead was obtained by melting up tin-foil picked up on the asylum grounds. The space in the lower half

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is filled with powdered orris-root, in which is placed the body of a locust.

The grace, beauty, and poetic conceptions shown in these sketches and drawings, and also in the quotations, are such that it must cause the most profound regret that such talent and originality should have been hampered in its development by a faulty physical development, and that an incurable mental disease should have clouded such a brilliant intellect.

As in the former article, it is not proposed to discuss the nature of the mental disease from which this remarkable and talented man is suffering, but merely to present as complete a record as possible of his disease. Since his transfer to Ward's Island there have been other and different developments of his artistic work, but the present article may properly end with the close of his residence at the Bloomingdale Asylum.

SOME EFFECTS OF ELECTRICALLY STIMULATING GANGLION CELLS.[1]

C. F. HODGE.

The experiments about to be described were made in the Psycho-physical Laboratory of the Johns Hopkins University during the winter of 1887-8 and fall and winter of 1888. I desire at the outset to make the fullest acknowledgment of my indebtedness to Dr. H. H. Donaldson, under whose guidance the work has been carried on, and to whom much of its success is due.

THEORY AND PURPOSE OF THE INVESTIGATION.

The theory that underlies physiology is that all function is due to chemical changes taking place within the living organism; and, further, that the functional activity of a specialized tissue depends primarily upon these changes in its individual cells. That these changes may reach a magnitude easily demonstrable with the microscope has been proved, as every one knows, for the cells of certain glands. My present purpose is to ascertain to what extent and by what methods changes due to the functional activity of ganglion cells may be in like manner demonstrated.

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[1] Dissertation submitted to the Board of University Studies, Johns Hopkins University, for the degree of Doctor of Philosophy, April, 1889.

A preliminary communication upon this work appeared in the AMERICAN JOURNAL OF PSYCHOLOGY for May, 1888.

HISTORICAL.

The great amount of work devoted to the morphology of the nerve cell is in the main aside from the problem in hand. It need be here referred to simply as it furnishes the structural mechanism necessary for a conception of the physiological working of the ganglion cell. This is furnished by Schultze,[1] to whom is justly ascribed the credit of demonstrating the fibrillar and granular structure of nerve cells. Later methods have, however, demonstrated this more sharply; and for the best of these we are chiefly indebted to Professor Kupffer[2] and Boveri.[3] This method consists, essentially, of hardening a perfectly fresh nerve in osmic acid and subsequently staining with acid fuchsin. Seen in section after this treatment, the medullary sheath appears black, and, what is of most interest here, the axis cylinder shows fine red fibrils in a finely granular matrix. The ganglion cell by this method is found to consist, beside nucleus and nucleolus, of a dense tangle of fibrils, unquestionably the same as those occurring in the axis cylinder, with an irregularly granular material filling the spaces between the fibrils. We are thus given at least the two things necessary for a nerve mechanism: the fibril to conduct, and, in close connection with this, some sort of substance, changes in which may serve to originate or modify the nerve impulse.

Of great significance to the problem in hand is such work as Heidenhain and Langley have done on the histology of resting and secreting glands. There are evident points of similarity between the active processes of gland and ganglion cells. There seem also to be points of difference which make it difficult at present to homologize the processes of the two. At any rate this is not the place to discuss that phase of the subject.

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[1] General characters of the structures composing the nervous system. Max Schultze, Stricker's Manual of Histology, p. 116.

[2] Ueber den Axencylinder markhaltiger Nervenfasern. Prof. C. Kupffer. (Sitzgb. d. math. phys. Klasse d. k. bayr. Akad. d. Wissensch. 1883, H. 3.)

[3] Beiträge zur Kenntniss der Nervenfasern. Theodor Boveri, Munich, 1885.

The pathology of nerve tissue is, of course, closely related to its functional activity. The subject, however, is so large, and so little of it bears directly upon the point in hand, that any general discussion of the pathological literature may be best relegated to a subsequent chapter. Two papers may be briefly referred to as throwing some light upon the results of my own experiments.

In 1878, Angelucci[1] made a study of the histological changes in spinal ganglion cells of four cases of nervous disease, one of chronic, two of acute myelitis, and one case of paralytic insanity. I will only note that in the series of degenerative changes described by him, the nucleus plays an important part. It early loses its rounded outline, becoming "stelliforme," shrinks up and disappears, leaving the cell a lump of pigment and fat.

More nearly physiological is some work of Rosenbach[2] upon histological changes in the ganglion cell due to hunger. Rosenbach worked upon dogs. His method consisted in depriving the animals of food for different lengths of time. At the expiration of the desired period, or upon the death of the animal, sections were made of the spinal ganglia and cord, and compared with similar preparations from well-fed dogs. Among other changes, and preceding degeneration or atrophy, he finds that the cells shrink and become vacuolated. Upon the death of the animal the nuclei had disappeared from many of the cells. The nerve fibers, however, appeared normal.

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[1] Osservazioni sulle alterazioni dei gangli intervertebrali in alcune malattie della midolla. Arnaldo Angelucci. Atti della R. Accademia de Lincei, Serie III^{a} V^{e} 2^{o}. Rome, 1878.

[2] Das Nervensystem im Hungerzustande. P. Rosenbach. Centralblatt für Nervenheilkunde, 1884.

But little has been done upon the purely physiological histology of the ganglion cell.

In 1869, Svierczewski[1] described changes in the living cells of the frog's sympathetic ganglia, changes in part probably due to functional activity of the cell. He kept the cells alive in aqueous humor or lymph under the microscope, and subjecting them to different conditions, observed the effects. It is significant to note that his attention centered upon the nucleus and its contents. The nucleoli were seen to wander about in the nucleus, sometimes in the most lively fashion, for as long as twenty-four hours. On exposing the cells to carbon dioxide, a finely granular precipitate suddenly formed within the nucleus, which redissolved on treatment with oxygen or hydrogen ("paraglobulin-reaction"). This process was accompanied, under certain conditions, by a marked shrinking of the nucleus, its rounded form being altered to an angular or "zick-zack" outline, the nucleolus being at the same time lost to view.

Somewhat similar observations were made by Freud[2] upon the living ganglion cells of Astacus. He describes shreds and angular-shaped particles which change form and position in the nucleus.

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[1] Zur Physiologie des Kerns und Kernkörperchens der Nervenzellen des Sympatheticus. Svierczewski. Centralblatt für die medicinischen Wissenschaften, 1869, p. 641.

[2] Ueber den Bau der Nervenfasern und Nervenzellen beim Flusskrebs. Wiener Sitzgb. 1882, p. 1.

Kühne,[1] in the course of his other studies, notes a fact which bears directly upon the present subject. This is a disarrangement and a shrinking together of the axis-cylinder fibrils of the nerves in the nictitating membrane of a frog, due to ten minutes unipolar stimulation of the nerve root within the skull. Also vacuoles make their appearance among the fibrils.

The only paper devoted to the exact problem under consideration has come out within the last month. The author states the exact question: "Is the activity of the central nervous system accompanied by changes recognizable with the microscope?"[2] He proceeds to answer the question under the idea that staining reveals much finer differences than changes of form. This determines his method, which consists in choosing two frogs of the same weight and sex, the one to be experimented with, the other for control. He then proceeds to stimulate by induction shocks the eighth nerve of one for one hour, keeping the control frog as quiet as possible during the same time. The spinal cords of both are hardened in corrosive sublimate solution and alcohol, and sections made through both cords opposite the origin of the eighth nerve. The sections are stained on the slide with haematoxylin, nigrosin, eosin, and safranin, the Gaule combination, in the order named. In *some* cases, the author states, sections of each cord are treated on the same slide. It is significant that here too the interest centers about the nuclei. These, by a difference of staining, fall into two categories, the red and the blue;

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[1] Neue Untersuchungen über motorische Nervenendigung. Kühne. Zeitschrift für Biologie, 1887, p. 56, Table D, Fig. 64.

[2] Wird der thätige Zustand des Centralnervensystems von microscopisch wahrzunehmenden Veränderungen begleitet? Bohdan Korybutt-Daszkiewicz. Archiv für mik. Anat. 1889, p. 51.

and a greater proportion of the nuclei stain red in the cord of the stimulated frog. A count of all the red and all the blue nuclei in a large number of sections shows that 3.31 to 3.66 times more nuclei stain red in the stimulated than in the unstimulated frog. The results are derived from four frogs, two stimulated and two control.

We gather from this brief résumé that nerve tissue of the frog, crawfish, dog and man has been examined with special reference to the observation of changes occurring in it. The main points noted up to date are, first, changes in the appearance of the living nucleus; second, vacuolation and shrinkage of the cell protoplasm and also of the axis cylinder; and third, that the nuclei in the spinal cord of a stimulated and unstimulated frog stain somewhat differently.

METHOD OF INVESTIGATION.

The value of results, especially in this branch of histology, depends so much upon the soundness of the method employed, that a somewhat detailed description of some features of my method must be given. I have used in all cases the spinal root ganglia. My scheme of procedure has been throughout to stimulate electrically a nerve going to one or more of these ganglia on one side of the animal, leaving the corresponding parts on the other side at rest. To avoid possible confusion, the right side was invariably used for stimulation, the left for control. At first a double control was used, consisting of the corresponding ganglia from a similar animal of the same size and sex. This practice was soon abandoned, however, for it was found that the ganglion cells of two frogs that could not be distinguished externally might differ very widely in staining and general appearance. The stimulated nerve was not divided, so that the contractions of the muscle supplied by it could be used to indicate the healthy condition of the nerve. If the nerve was conducting impulses peripherally to its muscle, it was taken for granted that it was conducting impulses in like manner centrally to its ganglion.

In general, as a means of stimulation, the ordinary combination was used, of Du Bois-Reymond coil, platinum electrodes, and bichromate or copper sulphate cell; and the strength of stimulus was determined within physiological limits by touching the electrodes to the tongue before beginning to stimulate. Special apparatus to regulate the strength of primary current and number of stimuli per second may best be described in connection with the purpose of special experiments. Intervals of rest were generally allowed. This was at first managed by having a key in the primary circuit and making and breaking the circuit by hand. Later, this part was relegated to clockwork, which spaced the intervals with more precision and removed the chief feature of irksomeness from the operation.

At the end of the desired length of time the stimulated ganglion, with its mate of the opposite side, was as quickly as possible excised and the process of fixing and hardening begun. The method from this point on is directed toward having the two ganglia pass through *identical treatment. In no single instance, no matter how many controls were used, were they separated from the time they left the animal to the time when, placed side by side upon the same slide, they were ready for study.* Not only were they carried through the same reagents, but, *in every case,* through the same reagents *in the same bottles or dishes, from the first fixing fluid to the solid parafin.* And from this point *the two are cut at the same stroke of the microtome knife,* fastened to the slide together, *stained together,* and appear side by side in the same field of the microscope.

All this was made easy by a simple device, which may be of use to others. The ganglia were usually left attached to their segment of the spinal cord until ready to pass into strong alcohol. They are then trimmed for cutting, and arranged in the same position relative to each other upon one end of a small strip of mica; 1x3 cm. is a convenient size. As they lie upon the slip, a drop of the thin white of an egg is placed over them. This is allowed to dry somewhat and the whole carefully laid in the alcohol, where the egg speedily coagulates, holding the ganglia firmly to each other and to the mica slip. The rule of always placing the stimulated ganglion nearest the end of the slip aids in simplifying matters. Any desired record may be scratched upon the other end of the slip. Not only one, but several pairs can be fastened to the same slip, arranged in a row so that they all may be cut at the same time. For example, it was my practice to stimulate the right brachial and sciatic plexuses of a frog. This places at our disposal five pairs of ganglia. These may be hardened by five different methods, and all be arranged as described above on a single slip of mica. They are all cut together, fastened to the slide together (invariably by the alcohol fixing method where differences of staining or granulation are to be studied), and all stained together. Many slides are obtainable from one such set of ganglia, and each slide may be stained in a different way. This device has given me, incidentally, a permutation of hardening and staining combinations which might well form the basis of a separate study.

In this way not only may a dozen specimens be manipulated as easily as one, but they are held in the desired positions relative to each other and, of special importance, they are cut together. However perfect the microtome, sections do not come from it of absolutely uniform thickness; and where minute, or even gross, differences of staining are to be studied, this is of prime importance.

Apropos of Korybutt-Daszkiewicz's work, I have sections, no thicker than his and obtained by essentially the same method, which show a most striking differentiation into red and blue nuclei. It requires but little focusing, however, to demonstrate that the red nuclei occupy the superficial, and the blue the deeper, layers of the section. A slight difference in the thickness of the section might thus change the proportions of the two quite materially. The thinner the section, according to the above, the larger would be the proportion of red nuclei. That this may be an explanation for Korybutt-Daszkiewicz's result is indicated by the fact that in equal areas of section he finds nearly 400 (4127 to 3759) nuclei less in the stimulated than in the control cords. This would suggest that the sections of the stimulated cords are thinner than those of the control; and from these he gets his preponderance of red nuclei.

The essential feature, then, of my method is that it compares *corresponding ganglia* of the *same animal* which have been subjected to *identical treatment* in passing from the animal to the slide; *the only point of difference being that the one has had its nerve stim-*

ulated for a longer or shorter time, while the other has not. Methods of hardening and staining do not concern us so long as the two ganglia go through every step of the processes together.

For the encouragement of others I may say that I have tried almost every method practicable and impracticable, in the hope of finding a striking reaction. Some such were found, but up to date they have all proved inconstant. Trzebinski[1] has made a special study of the influence of hardening reagents upon the ganglion cells of the spinal cord. He finds corrosive sublimate one of the best reagents, and states that it does not produce vacuolation of the cell. This method followed by Gaule's quadruple staining has furnished my best preparations for the study of granulation and staining. Trzebinski did not, it would seem, experiment with osmic acid. This, with fuchsin, safranin, or all four of the Gaule stains, has given a most beautiful preservation of the form of the nucleus and the minute structure of the cell protoplasm.

Two widely different animals, the frog and cat, were purposely selected to furnish the material for investigation. And the results which I will now consider are derived from fifteen experiments upon frogs and ten or eleven experiments upon cats. *All the experiments* will be referred to either singly or in groups.

RESULTS.

Frog No. 1 was given three drops of 1 per cent curare solution, and the sciatic nerve of the right side was stimulated continuously for thirty minutes. The three pairs of sciatic ganglia were excised and, with those of a control frog, hardened in corrosive sublimate. The ninth pairs were stained *in toto* in soda carmine; and for some unaccountable reason scarcely any nucleoli could be found in sections of the stimulated ganglion, while they appeared as usual in the ganglion of the other side and control ganglia. A count of the two gave the following:

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[1] Einiges über die Einwirkung der Härtungsmethoden auf die Beschaffenheit der Ganglienzellen im Rückenmark der Hunde und Kaninchen. Trzebinski. Virchow's Archiv, Vol. 107.

		Nuclei.	Nucleoli.
Six sections of each contained	Resting,	122	92
	Stimulated,	177	28

The seventh and eighth pairs stained in other ways (Kleinenberg's haematoxylin and by Weigert's method) gave no such result; and in fact the phenomenon could not be made to reappear in any subsequent experiment. Other than this, no results could be made out.

Three frogs were next taken, each with a control; each was given the same amount of curare, and the right sciatic nerves of the three were stimulated continuously one, two, and three hours respectively. From the nine stimulated ganglia thus obtained no effect of activity could be discerned.

An experiment was then made to test the influence of curare upon the working of the central portion of the reflex arc, and the indications seemed quite strong that, although curare does not entirely suspend nervous action in the cord, it does reduce the activity very materially. Its further use was for this reason abandoned.[1]

Frog No. 6 was used to demonstrate *post mortem* changes in the ganglion cells, and does not concern us now.

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[1] The proof that curare influenced the effect of stimulation is not conclusive, since continuous stimulation was also given up at the same time. Further experiment is needed on this point.

Frog No. 7 was made reflex, and the right brachial and sciatic plexuses were stimulated, with two minutes of rest alternating with two minutes of stimulation, for two and a half hours. The stimuli were regulated so as to be as strong as possible without causing reflex contractions of the muscles of the other side. At the end of this time but slight muscular contractions could be obtained from the arm or leg of the right side, and no reflex contractions whatever from stimulating the skin of this side, while stimulating the skin of the left side gave normal reflex contractions in that side.

Sections of the ganglia from the two sides show marked differences. Perhaps the most pronounced of these is a difference, noted independently by a number of observers, viz. that the nuclei appear shrunken in the stimulated ganglia. This led to a series of measurements, the results of which are given in the following table. The nuclei were measured, long and short diameters, in sets of one hundred, fifty stimulated and fifty unstimulated being taken from as nearly corresponding sections as possible of the two ganglia. A definite rule precluded any willful selection of the cells to be measured, the rule being that only nuclei containing nucleoli should be measured, and that all such should be taken in the order of their occurrence in the section. The measurements were made to the nearest μ under a magnifying power of Leitz, Oc. 3, Obj. 7 (= 600 diameters).

TABLE I.[1]

Frog No. 7. Made reflex, stimulated 2½ hours, intervals of rest and stimulation being two minutes. Three sets of 100 nuclei each.

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[1] The tables have been recomputed, and so differ slightly from those in the preliminary communication (AM. JOUR. PSYCHOL., Vol. I, p. 479 ff.). The difference is due to using the square root of the average surface of the nuclei, instead of the arithmetical mean, for the mean diameter.

			Nuclei in μ.		Cells in μ.
			Mean Diameters.		Mean Diameters.
Ganglia hardened in corrosive sublimate.	8th pair.	Resting	13.57	Set 1.	39.69
		Stimulated	12.23		35.00
		Diff.	1.34		
	9th pair.	Resting	13.94	Set 2.	
		Stimulated	12.56		
		Diff.	1.38		
	2d pair.	Resting	14.48	Set 3.	
		Stimulated	13.26		
		Diff.	1.22		

Sets 1, 2 and 3, volume shrinkage, 24 per cent.

The volume shrinkage per cent is computed from the mean diameters, treating the nuclei as spheres.

Besides shrinkage of nuclei, other changes are plainly visible. The protoplasm of the stimulated cells is much more vacuolated than that of the resting, and the staining, by Gaule's quadruple method, is less intense. Instead of the coarsely and densely granular constitution of the resting cell, we find the protoplasm of the stimulated cell finely granular and vacuolated. Owing in part to this absence of granules, the nuclei are more distinct in the stimulated cells. In part this is also due to a deeper staining of the nucleus itself, the open reticular appearance of the nucleus giving place to an evenly dense stain. There can be no doubt that in my preparations the stimulated nuclei stain much bluer than the resting, except in cases where pathological conditions were present.

The five experiments succeeding this were made with the purpose of getting the greatest amount of change possible; and under the supposition that this might be obtained, for the frog at least, in shorter time if the nutrition of the cells were prevented, the frogs were thoroughly bled or the capsules of the ganglia torn off. None of these experiments gave definite results. Sections of both ganglia stained by the above method appear redder than normal. This is presumably due to a clogging of the cells with decomposition products which would normally be carried away in the circulation. Stimulated and resting alike show vacuolation, probably the same as that found by Rosenbach in starving dogs. The nuclei in both appear shrunken, but do not show any marked difference in size.

Results of but a single experiment of this class need be given.

TABLE II.

Frog No. 8. Bled. Stimulated for 7 hours, five minutes of stimulation alternating with five minutes of rest. One set of 100 nuclei.

MEAN DIAMETERS IN μ.

Ganglia of 8th pair, hardened in corrosive sublimate and stained by the Gaule method.	Resting .....12.36 Stimulated ..12.01	Volume shrinkage, 8%.

One experiment, in which the ganglia were suspended in normal salt solution while being stimulated, gave more definite results.

TABLE III.

Frog No. 14. Sciatic ganglia of right side suspended in salt solution and stimulated 3½ hours, five stimuli per second, one minute of stimulation alternating with one minute of rest. The ganglia of left side kept during this time in blood of same frog. Two sets of 100 nuclei each.

MEAN DIAMETERS IN μ.

9th ganglia; corrosive sublimate and Gaule's staining.	Resting	14.70	Set 1. Measured by myself *previous* to Mr. W.'s measurement of set 2.
	Stimulated	13.10	
	Diff.	1.60	
	Resting	14.57	Set 2. Measured by Mr. W. *without knowledge of my results*, and having but one of the ganglia in the field at the same time, and *not knowing which had been stimulated and which not.*
	Stimulated	12.14	
	Diff.	2.43	

Sets 1 and 2, volume shrinkage, 36 per cent.

It was thought that greater changes might be obtained at a higher temperature; accordingly one experiment was made to test this, and though not altogether successful, the results may be given.

TABLE IV.

Frog No. 15. Cerebrum removed and wound allowed to heal before experiment. Stimulated 5½ hours at a temperature of 35° C.; intervals of rest and stimulation being one minute. Three sets of 100 nuclei each.

MEAN DIAMETERS IN μ.

Ganglia of 2d pair. Hardened in picric acid. Gaule's stain.	Resting	16.53	Set 1. Measured by myself *previous* to measurement of set 2.
	Stimulated	15.66	
	Diff.	.87	
	Resting	17.40	Set 2. Measured by Mr. L. without knowledge of my own measurement, and *not knowing which of the ganglia had been stimulated.*
	Stimulated	15.84	
	Diff.	1.56	
9th pair. Flemming. Gaule's stain.	Resting	20.90	Set 3.
	Stimulated	19.13	
	Diff.	1.77	

Sets 1, 2 and 3, volume shrinkage, 12.5 per cent.

It is to be noted that both Mr. W.'s and Mr. L.'s measurements make the difference greater than my own. Staining and structure of protoplasm not well defined; due probably to the fact that the frog died toward end of experiment. At its close the muscles were beginning to pass into *rigor mortis.*

This closes the series of experiments upon frogs. It is desirable, for a more general application of the results above detailed, to ascertain whether they hold good for some other animal. Experience has shown that the most marked results are to be expected from experiments in which the condition of the animal is most nearly normal. I think I am justified in distrusting the influence of curare; from the following experiment, chloroform also would seem to be a disturbing factor. A cat was killed with chloroform, and several of the spinal ganglia were examined. Many of the cells were found to show considerable vacuolation, strikingly similar to that produced by stimulation. The point needs further investigation, but it is not altogether improbable that a substance which produces such marked physiological effects may also give rise to histological changes in nerve centers. However this may be, it was determined not to run any risk of complicating matters by the use of anaesthetics. A mode of producing insensibility without the use of drugs was accordingly resorted to.

The method[1] adopted consists in trephining the skull at about the point of greatest convexity and a centimeter to one side of the middle line. A small slit is made in the dura, and through this a blunt glass rod is thrust directly to the floor of the skull, and worked carefully across along the floor to the opposite side. The crura are thus severed at their entrance to the cerebrum; and if successful, complete anaesthesia, with normal pulse and respiration, should result. The operation is performed while the animal is under slight anaesthesia from ether. The right brachial plexus is then laid bare in the axilla, and, with great care as regards injuring the nerves or blood-vessels going to them, freed from fat and disentangled from the subclavian artery and vein, so that these may not be included between the electrodes. By including the whole plexus at this point, four ganglia are stimulated. My own practice has been to slip a two-tined platinum electrode over the plexus from behind, in such a way that the two tines of the electrode touch opposite sides of the nerves and make it necessary for the stimulus to pass obliquely through them. The greater number of the fibers, however, from the 6th and 7th cervical escape stimulation, and possibly, too, the nerves from the 1st dorsal and 8th cervical coming first between the electrodes, tends somewhat to short circuit the current, thus depriving the other nerves of an equal share of the stimulation. At any rate, the 6th and 7th cervical have failed to show the effect of stimulation to the extent that the 8th cervical and 1st dorsal do. Hence for the clearest results it is best to include in the circuit only the medius and spiralis nerves and the branches lying behind these, and then use only the 8th cervical and 1st thoracic ganglia. A double control was employed at first, consisting of thoracic ganglia from the same animal, a pair of these being carried through with each pair of test ganglia. This was soon found to be entirely unnecessary, since the cells of these control ganglia invariably resembled those of the resting ganglion.

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[1] The method was obtained from a paper entitled "On the Renal Circulation during Fever" (Walter Mendelson, Am. J. M. Sc., Phila. 1883), where the method is credited to Ludwig.

TABLE V.

Cat No. 1. Stimulated for 7 hours; intervals, one minute.

Ganglia of—	Nuclei (four sets 100 each).			Cells.
		Mean Diameters in μ.	Shrinkage. In Volume.	Mean Diameters in μ.
1st thoracic. Hardened in osmic acid.	Resting	16.29		59.06
	Stimulated	14.07	35%	57.19
	Diff.	2.22		
8th cervical. Hardened in corrosive sublimate.	Resting	14.91	(Selected).	All over 50 μ.
	Stimulated	11.70	51% (T.)†	
	Diff.	3.21		
7th cervical. Hardened in Flemming's fluid.	Resting	16.60		57.50
	Stimulated	15.41	20%	56.25
	Diff.	1.19		
6th cervical. Hardened in picric acid.	Resting	14.98		44.21
	Stimulated	14.23	14.6%	44.74
	Diff.	.75		

Stimulation severe, frequently varied and regulated so as to produce the greatest amount possible of muscular contraction in the right fore-leg without giving rise to reflex contractions in other parts of the animal. Muscular contractions in right fore-leg toward close of experiment feeble but constant. Within five minutes after the animal was bled the muscles of this leg had passed into *rigor mortis*, the muscles of other limbs being normal and irritable. Pulse and respiration remained normal the whole time.

Besides shrinkage of the nuclei, other important changes occur. For the first thoracic pair, hardened in osmic acid, the nuclei are plump and round in the resting ganglia and stain lighter than the protoplasm. In the stimulated ganglion they are irregular in outline and stain much darker than the rest of the cell. Especially marked in this case also is the extreme vacuolation of protoplasm in the stimulated cells, and this does not occur in the ganglion of the other side or in the two thoracic ganglia used as control. It was noticed independently by three observers that the nuclei of the cell capsule were shrunken in the stimulated ganglion. (See Figs. 3 and 4 of plate.) The eighth cervical ganglia, hardened in corrosive sublimate, show the characteristic appearance of the nuclei, with slight difference in the staining of the protoplasm.

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† See explanation page 395, note.

TABLE VI.

Cat No. 2. Stimulated 1 hour 40 minutes; intervals, one minute.

		100 NUCLEI.		100 CELLS.
		Mean Diameters in μ.	Shrinkage in Volume.	Mean Diameters in μ.
Ganglia of 1st thoracic. Osmic acid.	Resting	14.91	25.6%	14.94
	Stimulated	13.51		14.45
	Diff.	1.40		

Examination of the sections shows changes similar to those described for cat No. 1, but less in degree.

Though no attempt was made to render the stimulation equal for cats No. 1 and 2, it is hinted by the results of the two experiments that some sort of a quantitative relation exists between amount of stimulation and amount of change in the cells. Such a relation should obtain if we are dealing with cause and effect. To test this point with theoretical precision is, of course, impossible, for we must know, in order to do this, not only the strength of stimulus used, but also that the same amount of stimulation is distributed to the same number of cells; and, further, that the ganglion cells of one animal are exactly as irritable as those of another animal. In the following series, therefore, we assume that the irritability of cats is in general the same, and that the same nerves in different cats connect approximately with the same number of ganglion cells. To render these factors as nearly alike as possible, half-grown kittens were used throughout. The amount of stimulation was regulated by using the same apparatus for all the experiments, and by placing a rheocord, resistance-box and galvanometer in the primary circuit derived from two one-liter copper sulphate cells. By manipulation of the resistance-box and rheocord, the galvanometer needle was brought to a given position and held at this point during each experiment of the series. The results require little more than tabulation.

TABLE VII.

SERIES WITH EQUAL STIMULATION.

		Length of Stimulation.	No. of Nuclei Measured.	Mean Diameters of Nuclei in μ.	Shrinkage in Area of Section of Nucleus.	Shrinkage in Volume of Nucleus.
Intervals of 15 seconds of stimulation alternate with 45 seconds rest. Ganglia of 1st thoracic pair, hardened in osmic acid.	*Cat No.* 7 (operated upon and left without stimulation for 2½ hrs.)...........	0 hrs.	100	14.20 14.54	—4.7%	*—6.9%
	Cat No. 5..........	1 "	100	14.70 13.51	+14.1%	+22%
	Cat No. 6..........	2½ "	200 (T.)†	11.86 10.95	+13.8%	+21%
	Cat No. 8..........	5 "	100	15.97 14.38	+17.1%	+24.3%
	Cat No. 11.........	10 "	100	16.19 13.35	+31%	+43.9%
		‡S.	100			
		S.	100 (T.)			
			100 (T.)			

---

* The minus sign indicates that the nuclei of the right ganglion are larger in this case. In the only other set measured from a normal pair, the nuclei were also slightly larger in the ganglion of the right side.

† Sets marked T. are measured by a third person, with whom every precaution was taken to obtain a purely mechanical and unprejudiced measurement.

‡ Sets marked S. are those in which only nuclei in cells of over 50 μ diameter were measured.

Two Experiments with Stronger Stimulation.

		Time.	No. of Nuclei Measured.	Mean Diam.	Area Shrinkage.	Volume of Shrinkage.
1st thoracic ganglia. Osmic acid.	*Cat No.* 9 (45 seconds rest to 15 seconds work)............	2 hrs.	100	12.39 10.45	31%	40.9%
	Cat No. 10 (intervals 15 seconds rest to 15 seconds work).	2 hrs.	100 (T.)	13.82 12.04	23.9%	32.7%

In the above series the stimulus used was very slight, too slight for the most definite results. Still no special study of the sections is necessary to detect the gradation expressed in the table, staining as above described. The use of the one reagent throughout the series has emphasized the fact that osmic acid is not so strongly reduced by the cells of the stimulated ganglia. This sometimes results in so marked a difference in staining that sections of the two ganglia may be easily distinguished by the naked eye. The vacuolation, so striking in the stimulated cells of No. 1, is only slightly present in these experiments, presumably because the stimuli were not strong enough.

Many devices were resorted to to eliminate the personal equation and obtain mechanical measurements. Three persons unacquainted with the methods of the investigation have kindly consented to assist in the work of measuring. Even here the differences are too marked to make an absolutely neutral state of mind possible, since each of the three, before completing the measurement of a single set, had a very definite notion that the nuclei in the two ganglia were different. In my own measurements I was wont from the first to throw all thought of the work as completely as possible off my mind, to think about something else, to have a story in which I was interested read aloud, or something of the kind to divert my attention. In general also all the measurements were made before the results were footed up, so that the way they were tending could have no unconscious influence upon the measurements.

The objection has been raised that the changes above described, especially shrinkage of the nucleus, may be pathological. It is true they resemble changes hitherto described as pathological; but up to the present no attempt has been made to distinguish changes due to fatigue from those caused by disease, and on *a priori* grounds we should expect the former to precede and shade into the latter. The fact that the change becomes steadily greater as the stimulation is prolonged, would further indicate that it is due to active processes of the living cell. It would be interesting to know whether stimulated cells will return to the normal condition if given a sufficiently long period of rest. But whether they do this, or whether they die and give place to new cells, is a point for future investigation, and not the question in hand. In either case we are safe in assuming that the changes are such as occur in the normal working of the ganglion cell.

Conclusion.

A method has been attained by which changes due to functional activity can be as easily and certainly demonstrated in a ganglion as in a gland. The chief of these changes for the spinal ganglion cells of the frog and cat are:

As a result of electrical stimulation:

A. For the nucleus: 1. Marked decrease in size.

2. Change from a smooth and rounded to a jagged, irregular outline. 3. Loss of open reticular appearance with darker stain.

B. For the cell protoplasm: 1. Slight shrinkage in size. 2. Lessened power to stain or to reduce osmic acid. 3. Vacuolation.

C. For the cell capsule: Decrease in size of the nuclei.

INCIDENTAL OBSERVATIONS.

A number of suggestive observations, made in the course of the investigation and not belonging properly to the body of the paper, may be mentioned here.

A strange differentiation of some sort between the large and small cells of the spinal ganglia is brought to light by stimulation. The large cells show the effects of work; the small cells, very little or not at all. The fact is too marked to pass by unnoticed. Considering all the cells large which have one diameter 50 μ or over, and those small which have not, a count gives the following result:

Cat No. 11. *First Thoracic Ganglia.*

	In 100 large cells nuclei:		In 100 small cells nuclei:	
	Shrunken.	Not shrunken.	Shrunken.	Not shrunken.
Resting,	5	95	0	100
Stimulated,	94	6	8	92

A few fibers going to a ganglion of course escape stimulation by our method. This accounts very well for the few large cells which do not appear worked in the stimulated ganglion. It cannot account for this condition in the multitude of small cells which comprise over half the cells in a ganglion. No explanation will be attempted until further experiment is made. As might be expected, a few cells in the resting ganglion appear worked.

In close relation to our work is the question of the minute structure of a spinal ganglion. What is the path of a nerve impulse through the ganglion? The supposition has been, since Ranvier's work, that a fiber enters a spinal ganglion, unites with one of its cells by a "T" process, and passes out in the opposite direction. This supposition finds support in the way a nerve degenerates when separated from the ganglion, and in the fact that the same number of fibers enter a ganglion as leave it.[1] It is also supported by Birge's[2] work, in which he finds a single ganglion cell in the anterior horn of the frog's spinal cord for each nerve fiber in the anterior root. If this relation holds for the cells and fibers of the spinal ganglion, we should evidently find a cell in the ganglion for each fiber in the posterior root. Expecting to demonstrate this, Dr. Nelson,[3] working under the direction of Birge in the University of Wisconsin, counted the fibers in the posterior root and the cells in the corresponding ganglion. The work was done on the frog, and in all about ten ganglia were counted. Nelson found, allowing 2–4 per cent for error in counting, *two* ganglion cells for every fiber in the root. This counting has been repeated for two ganglia by myself, and for one more by still another observer. Our figures are as follows:

	No. fibers in root.	No. of cells in ganglion.
Seventh ganglion, right side,	1128	2767
Eighth " left "	1811	5416
Seventh " left "	1340 (T.'s count) 1364 (my count)	4456 (T.)†

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[1] Ueber den Bau der Spinalganglien. M. Hall. Wiener Acad. Sitzungsber. LXXII, 3, p. 31–37.

[2] Die Zahl der Nervenfasern und der motorischen Ganglienzellen im Rückenmark des Frosches. E. A. Birge. Archiv für Anat. und Phys. 1882, Physiol. Abth. p. 435.

[3] The above is inserted by the courtesy of Dr. Nelson. His notes of the work were destroyed in the burning of Science Hall, Madison, Wisconsin.

† The most careful count of all, done under magnifying power of Leitz, Obj. 7, Oc. 3 (= 600 diameters) for fibers, and Obj. 7, Oc. 1 = 325 diameters for cells.

The method consisted in counting all the nucleoli *in every section of a complete series through* the ganglion, and all the fibers in a cross section of the root between the ganglion and the cord, generally close to the ganglion. The tissue is hardened in osmic acid. A nucleolus may be pushed to one side or dragged out of a cell by the edge of the knife, but is never cut in two. So we run no risk on that score of counting a cell twice; and double nucleoli are so rare that they may be left out of the account.[1] My rule throughout was to count everything that could be construed to be a fiber in the root, and nothing but what was most certainly a cell in the ganglion, thus throwing all the doubtful cases upon the same side.

With the above figures approximately correct, either apolar cells in the ganglia must be very numerous, or the relations of fibers to cells must be more complex than formerly supposed. A most careful teasing of spinal ganglia of a frog, using a fine jet of water instead of needles, I think demonstrates the following points:

1. Apolar cells do not occur in the spinal ganglia of the frog in any considerable numbers, none having been observed.

2. Typical bipolar cells do occur. Three have been noted up to date.

3. The axis cylinder of the cell process is often seen to divide and enter the cell as a spiral and straight fiber.

4. At the angles of the "T" the axis cylinder of the cell process may be observed to divide and pass

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[1] In counting the last ganglion, T. kept careful account of all double nucleoli. In the 4456 cells 38 were found. In all cases they were found in the small cells, lying among the fibers or close to the nerve fiber axis of the ganglion.

both ways in the nerve fiber, of which it *seldom* forms the whole of the axis cylinder.

5. Two cases of double —+— have been found.

6. Two cells, in a number of cases, have been found to unite their processes, not necessarily as a cell junction, but to aid in making the axis cylinder of the same nerve fiber.

No rigor has been spared in this teasing from fear of breaking specimens. The coverslip has been tapped and each specimen rolled over and over while under the microscope until every point in the above description has been clearly demonstrated. A special investigation of these points is under way.

In the course of examining so many cells, an appearance of the nucleus has been noticed which may throw some light upon that most vexed problem, the minute structure of the ganglion cell itself. The marked effect upon the nucleus of stimulation would indicate an intimate relation between the nerve fiber and the nucleus. In general, the jagged points of a worked nucleus give the impression that it is connected at these points with the fibrillar reticulum of the cell protoplasm. At times, and not so very rarely, something more definite makes its appearance. A stream of fibrils is plainly seen to pass from one side of the nucleus to mingle with the fibrils of the cell. These fibrils arise from the nuclear membrane, and in no case have I been able to trace them to an origin within the nucleus.

March 15, 1889.

(For explanation of plate see page 402.)

EXPLANATION OF PLATE.

Fig. 1.—Cat No. [illegible]. Left 1st thoracic ganglion, resting. A portion of single field. Nuclei full and round, protoplasm darkly stained.

Fig. 2.—Cat No. [illegible]. Right 1st thoracic ganglion, worked, not severely, [illegible] hours. Cells *a* and *b* are adjoining cells, taken from a slightly more central portion of the section, and show an appearance of the nucleus, shrunken away from the cell protoplasm, quite characteristic for central portions of the worked ganglion, but not of the resting.

Fig. 3.—Cat No. [illegible]. Cell from left 1st thoracic ganglion, resting. Adjoining cells outlined in part.

Fig. 4.—Cat No. [illegible]. Cell from right 1st thoracic ganglion, worked severely [illegible] hours. Nucleus much shrunken and protoplasm vacuolated. [illegible] and [illegible], two larger vacuoles.

Note.—All the above figures were drawn under a magnification of Leitz Obj. [illegible], Oc. [illegible] (= [illegible] diameters), and the cells, nuclei and nuclei of capsule were outlined by the aid of a Leitz camera lucida after Abbe, the form with the longer arm.

Figs. 1 and 2, staining with [illegible] acid.

Figs. 3 and 4, staining with [illegible] acid, followed by Gaule's quadruple stain.

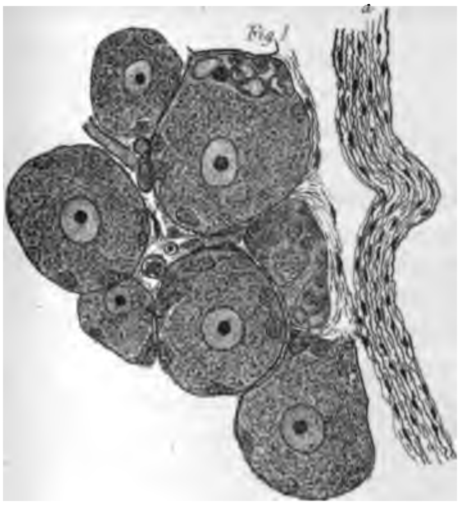


Fig. 1

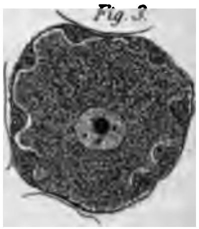


Fig. 3.

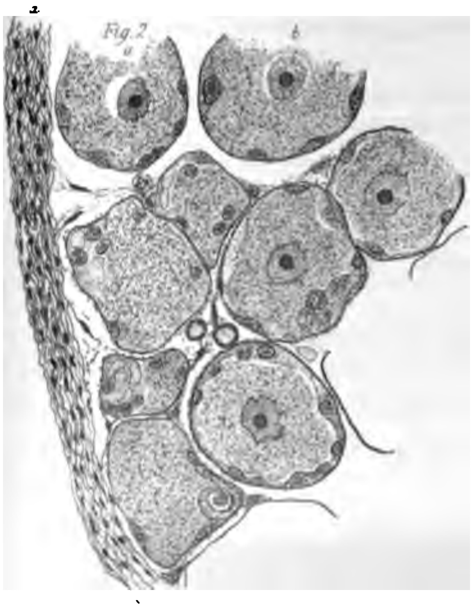


Fig. 2

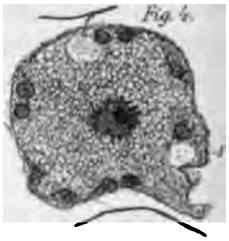


Fig. 4.

C. F. Hodge Del.

A. Hoen & Co. Lith. Balto. Md.

EXPLANATION OF PLATE.

Fig. 1.—*Cat No.* 11. Left 1st thoracic ganglion, resting. A portion of single field. Nuclei full and round, protoplasm darkly stained.

Fig. 2.—*Cat No.* 11. Right 1st thoracic ganglion worked, not severely, 10 hours. Cells *a* and *b* are adjoining cells, taken from a slightly more central portion of the section, and show an appearance of the nucleus, shrunken away from the cell protoplasm, quite characteristic for central portions of the worked ganglion, but not of the resting.

Fig. 3.—*Cat No.* 1. Cell from left 1st thoracic ganglion, *resting*. Adjoining cells outlined in part.

Fig. 4.—*Cat No.* 1. Cell from right 1st thoracic ganglion, *worked severely 7 hours*. Nucleus much shrunken and protoplasm vacuolated. 1 and 1′ two larger vacuoles.

Note.—All the above figures were drawn under a magnification of Leitz Obj. 7, Oc. 3 (=600 diameters), and the cells, nuclei and nuclei of capsule were outlined by the aid of a Zeiss camera lucida after Abbe (the form with the longer arm).

Figs. 1 and 2, staining with osmic acid.

Figs. 3 and 4, staining with osmic acid followed by Gaule's quadruple stain.

Fig. 1

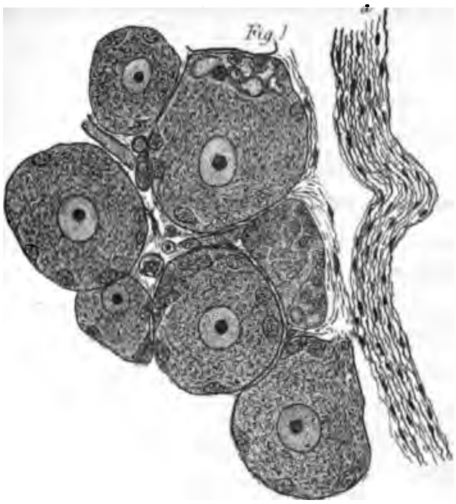


Fig. 3.

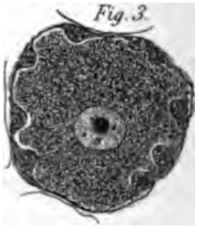


Fig. 2

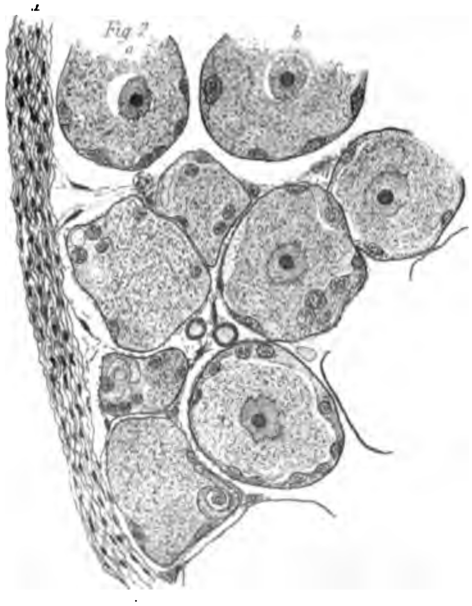
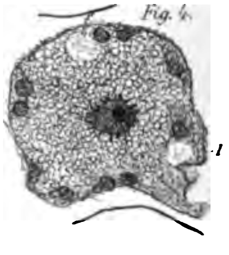


Fig. 4.



C. F. HODGE, Del.

A. Hoen & Co. Lith. Balt. Md.

PERSONAL EQUATION.

EDMUND C. SANFORD, PH. D.

III.

THE NATURE AND CAUSE OF PERSONAL EQUATION.

In the preceding section of this study, a number of conditions were set forth that may cause the personal equation to vary. In this I propose to exclude all such, and to consider the personal equation in itself under the simplest conditions. It is evident at once that even so, there will be room for classification. The personal equation differs in the two methods of observation, chronographic and eye and ear, and in the individual applications of each. I shall begin with the chronographic, because from a psychological standpoint it is the simplest; and with the observation of a sudden phenomenon, the precise instant of which cannot be foreknown; for example, the emergence of a star from the dark edge of the moon, or a heliotrope signal.

Identically the same thing has been studied by the physiological psychologists under the name of "reaction-times without signal."[1] A stimulus is received by the sense organ (here the retina); the impulse is conducted to the brain; brain changes result, accompanied on the psychic side by perception, apperception and volition;[1] a motor impulse starts out along the nerves of the arm; the muscular motion of tapping the key is executed; and the reaction is complete. The whole requires over a quarter of a second.[2] The portion to be credited to each stage is still extremely uncertain, but the sense organ and the muscles have short latent periods, the nervous conduction takes time, and the brain changes are very likely slowest of all. The absolute personal equations for this kind of observing should be 0.30 s or over; and, since no one can observe the event before it happens, the relative should be reduced to a few hundredths of a second.

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[1] Wundt: Physiologische Psychologie, 3te Aufl., II, 287. It is customary in most reaction-time experiments to give warning of the approach of the stimulus to be observed, at a short but variable time before its occurrence.

When the instant of the phenomenon can be foreseen with some exactness, as in transit observations, the process is a little different. The two ways of applying the method (waiting for the bisection, or anticipating it) must further be distinguished. The first corresponds closely to the "reaction-times with signal" of the psychologists, the steady approach to the wire giving most exact warning of the time to record. Reaction-times with a signal, as shown by the little table below, are shorter than those without—in gen-

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[1] Perception, apperception and volition mean here respectively, the first consciousness that a sensation is reaching the sensorium, the clear consciousness of the sensation, and the determination to react to it. For a practiced observer reacting to a stimulus of moderate intensity, the last two stages are more or less overlapped.

[2] Wundt gives values for reaction to the sound of a falling ball, with and without signal, as follows:

	Fall of 5 cm.	Fall of 25 cm.
	s.	s.
Without signal,	0.266	0.253
With signal,	0.175	0.076

The reaction-time for light is in general greater than that for sound. The experiments of Renz (*Astr. Nachrichten*, CXIX, 1888, 145) give values for the disappearance and reappearance of an artificial star varying from 0.40 to 0.60 s as the star declines in brightness.

eral, because the signal gives a chance for focusing the attention, tensing up the muscles, etc., and in particular because, when the stimulus is not too faint, it favors the use of the "motor form of reaction."[1]

In this form the attention is turned not to the stimulus, but with extreme concentration to the execution of the reaction; and there is a corresponding tension in the reacting member. The times with this form are about 0.1 s shorter than when the attention is concentrated on the stimulus, even when that is preceded by a signal, that is, for light, from 0.15 to 0.19 s. Wundt considers that when fully established, reaction after this form is probably an acquired brain reflex. The sensation on its entrance into the general field of consciousness at once releases the motor impulse without the mediation of the will, and the apperception of it takes place while the response to it is in execution. The extra-cerebral stages are as before, and may be assumed to be the same in time, except for the additional muscle tension. I am not able to say how frequent this form of reaction is among astronomers, but it is largely a matter of practice and may be frequent among them.

The observers that anticipate introduce still another element. Instead of reacting to an independent sensory stimulus, they react to a stimulus as affected by judgment—that is to say, they tap the key when they see the star in such a position that they judge from its rate and distance that it will be bisected when the key is tapped. The judgment is of course only semiconscious, if conscious at all, but nevertheless maintains its character. The effect of this way of using the chronographic method in varying conditions has already been shown. It certainly adds a rather complex psychological process to those before mentioned, though not one that need make any great difference in accuracy while conditions remain the same.

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[1] Wundt, op. cit. II, 265–267.

It is hardly necessary to point out that in all these cases the direct cause of the personal equation is the physiological inertia of the observer. To this the anticipators add a mental factor that compensates partially or wholly, or over-compensates, the physiological delay. Astronomers can therefore hardly hope to free their chronographic observations absolutely from the variations of personal equation. The nearest approach is probably to be found in the use of suitable artificial transits at the time and under the circumstances of the observations to be corrected.

THE EYE AND EAR METHOD.

In this method the processes are more complicated than in the last. Instead of executing a definite and predetermined motion in response to a definite sensation, the observer has here to fix disparate sensations with reference to one another—a much more difficult thing to do and one more open to illusion. The greater difficulty is witnessed by the larger probable errors and personal differences that attend this method.[1]

Let me first take up as before the observation of instantaneous phenomena. It is more than likely that in this method, as in the other, foreknowledge of the time at which the phenomenon is to appear, affects the observing of it; but such figures as I have found are insufficient to establish such a difference, and I must therefore speak of both cases together.

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[1] AMERICAN JOURNAL OF PSYCHOLOGY, II, 20–21 and 272.

The absolute error in observations of this kind has been measured by Renz with artificial stars.[1] In the preliminary report published the average values are irregular and for the most part do not exceed a few hundredths of a second. Whether this same result would be found with longer series and on other individuals remains to be seen. Determinations of relative personal difference in observing a sudden sound were made by Encke.[2] The mean result of two series was as follows:

$$\begin{aligned} E-B&=+\overset{s.}{0}.261\\ E-F&=+0.031\\ E-Q&=+0.083 \end{aligned}$$

The observer B, before knowing the result of the observations, remarked that he felt he had observed much too soon. The mean of two series taken later showed him in the right:

$$\begin{aligned} E-B&=+\overset{s.}{0}.026\\ E-F&=+0.032 \end{aligned}$$

Later still, observers B and Q found as the mean of three series of comparisons:

$$Q-B=+\overset{s.}{0}.130$$

The mechanism of the observation is easy to understand. The observer receives and counts the regular series of auditory sensations, and into this at some point is shot a sensation of sight.[3] The auditory sensations are practically instantaneous, that of sight leaves an after-image. The senses operate quite as usual, and we must look elsewhere for the largest part of the error. One can follow the rhythm of the seconds with very great accuracy; it is the entrance of the new stimulus, and, as I hope will appear before the end of the paper, the disturbance of attention thus produced that are really to blame.

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[1] *Astr. Nachrichten*, CXIX, 1888, 145.

[2] Abhandlungen der Berliner Akademie, 1858.

[3] It makes no great difference whether the observer attends at the instant of observation to a subjective clock-beat, as Wolf thinks, nor whether he counts mentally; that there should be subjective hearing and subjective counting is sufficient.

The sight stimulus may fall either very near one of the auditory series or in the midst between two, and there is room for some difference in the result according to which happens: if the first, the persistence of the sensations is involved; if the second, the estimate of time. On the first, some light is thrown by experiments of Exner, the Vienna physiologist.[1]

Exner's problem was: How far apart in time must two stimuli be in order to be perceived as successive? The first part of his study deals with stimuli to the same sense; for sight he used electric sparks or little illuminated holes seen one after another through a slit in a revolving disk; and for hearing, the clicks of a lead slip held against a toothed wheel. The next—and these experiments are most like the observations in question—refers to stimuli to different senses. His apparatus furnished a bell stroke and an electric spark, or a shock and a spark, of which the order and separation in time could be varied by an assistant. The observer was required to say whether the sensations were simultaneous, and if not, which came first. The couple were apparently repeated a number of times in the same order before a judgment was made. There are not many experiments reported, and the results are not easy to gather in statistical shape, but they show that "if a sense impression acts simultaneously on both eye and ear, the sensation of hearing will be perceived earlier than the sensation of sight," and they further give the basis for the second part of the table of approximate times (Exner himself as observer), with which he concludes that section of his article. I extract from his table those values that are of interest in this connection; the double stimuli are supposed to occur in the order given.

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[1] Pflüger's Archiv, Bd. XI, pp. 422–428.

	s.
Two noises	0.002
Spark images at the center of the retina	0.044
Bright disks " " " "	0.045
Noises at the two ears	0.064
Hearing—sight	0.06
Sight—hearing	0.16
Touch—sight	0.053
Sight—touch	0.071

In this table it is to be noticed that the time is lengthened when the stimuli reach different senses (or even different sense organs, in the case of the ears); and further, that the times are longer when sight leads, because of the longer after-sensation in that sense, and longest of all when sight leads and hearing follows.[1] The hypothesis that the difference depends largely on the adjustment of attention is supported by Exner's subjective observations. He thus describes the subjective side of the double stimuli experiments:

"A second way of adjusting consists in the following: We adjust for a definite one of the two stimuli, *e. g.* for a bell stroke in connection with the state of the sensorium at the instant of this bell stroke.[2] Then the other stimulus, *e. g.* the electric spark, comes into relation with the first either as preceding or following. One circumstance is here highly disturbing. The impression that is not adjusted for is much weaker in the memory-image than the one adjusted for; it is somewhat indistinct, poorly fixed as to time. There is, therefore, great uncertainty in this method. Usually the inclination is to take the subjectively stronger stimulus, the one adjusted for, for the first, just as the inclination is to take a considerably stronger objective stimulus for the first. Still the whole circumstance can also be reversed; it was in the experiments between touch and sight. There it often seemed to me as if the impression not fixed had already taken place at the time of the fixed; the phenomenon can suddenly change over again into the usual form. A similar phenomenon has been described by Wundt.[1] He states that between sight and hearing he can voluntarily perceive one or the other stimulus earlier according as he turns his attention to them. I have never succeeded with this combination, and the physiological delay of the sight impression comes into consideration, which Wundt had not yet recognized."[2]

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[1] In experiments with hearing and touch, the perception of succession was much more exact, but the amount of separation necessary did not appear.

[2] In speaking of the first way of adjusting he had said: "We adjust the attention for the first sense stimulus that is to strike us, without knowing which it is, of course. Not for this alone, however, but for the state of the sensorium—I cannot express myself differently—at the instant of the first sense stimulus."

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[1] Vorlesungen über Menschen- und Thierseele, Leipzig, 1863, I, 39.

[2] In the last edition of his *Physiologische Psychologie*, Wundt makes a similar statement. In speaking of the effect on the simple reaction-time of introducing a stimulus to another sense at almost the same instant as that to be reacted to, he says (II, 294): "Provisionally it may be remarked that the succession of our sense perceptions does not necessarily agree in order with the succession of sense stimuli, but that an actually later impression can readily be anticipated. Introspection leaves no doubt as to the origin of these illusions; they rest on the varying tension of attention. As soon as the tension for the chief sensation has risen to a certain point, it can bring that sensation, even when it actually follows somewhat later than the accompanying stimulus, in spite of that, into the focus of consciousness at the same time or earlier. The greater the attention the more marked will be the difference in time that can be overcome by it." See also II, 339.

It is evident, of course, that so long as a subject adjusted indifferently now for one sensation and now for the other, his observations would show accidental errors only; but if for any reason he habitually attended to one or the other, a constant error might enter and he would show a personal equation.

It may be mentioned in this connection that Encke found a small personal difference in observing the coincidences of the beats of two clocks running at slightly different rates,[1] and Littrow reports experiments of Weiss's to similar effect.[2]

All this, however, applies only when the phenomenon to be observed falls near one of the second beats. When it falls in the middle of the second, another service is required of the mind, one that probably introduces another error, namely, the estimation of the fractional parts of the second. Unfortunately there are no experimental determinations, so far as I am aware, of this factor. The time-sense has been studied somewhat, but with a different end in view. It may be conjectured that here also attention plays an important role. The grounds of this conjecture and of what has already been said about the function of attention will be somewhat strengthened by what is to be said in the next section, though the reference is not direct enough to justify repetition.

TRANSIT OBSERVATIONS BY THE EYE AND EAR METHOD.

Much more important, from an astronomical point of view, than the last, is the application of the eye and ear method to the observation of transits. The operations are here very different from those just considered, and more uniform in results, though still attended by considerable personal differences. The effort is to fix two points in the continuous line of motion of the star, one after and one before the crossing of the wire, which shall correspond with successive beats of the clock, to estimate the position of the wire between the two in tenths of the whole space, and thus finally to arrive at the time of the crossing in tenths of a second. The stage at which the important personal differences seem to enter is the connecting of the clock-beat with the position of the star.

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[1] Abhandlungen der Berliner Akademie, 1858.

[2] *Astr. Nachrichten*, LXVIII, 1867, 369.

The first and chief theory put forward in explanation of these is that of Bessel, given in his original study.[1] He says:

"These different experiments show that no observer, even if he believes he follows Bradley's method of observation in all strictness, can be sure to tell the *absolute* instant of time correctly. If it is assumed that impressions on the eye and the ear cannot be compared with each other in an instant, and that two observers use *different* times for the carrying over of the one impression upon the other, a difference originates—yet a greater, indeed, if one goes over from seeing to hearing, the other from hearing to seeing. That different kinds of observation should be able to alter this difference need not seem strange, if one assumes as probable that an impression on one of two senses *alone* will be perceived either quite or nearly in the same instant that it happens, and that only the entrance of a second impression produces a disturbance which varies according to the differing nature of the latter. It is probable that Maskelyne would have agreed more nearly with me than with Struve, Walbeck or Argelander, since in the opposite case a difference of about two seconds would have occurred between Kinnebrook and me, which is surely too great to be considered possible. Maskelyne and I therefore probably follow the custom of going over from hearing the second-beat to seeing, while the astronomers observing later appear to have accepted the contrary."[1]

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[1] Königsberg Observations, Abth. VIII, 1822, iii; also his Abhandlungen, Bd. III, p. 303.

Bessel's theory was suggested no doubt by his own mental processes in observing, and probably has commended itself to astronomers for similar reasons. Hartmann noticed something of the kind in his experiments,[2] and C. Wolf likewise, at least in his earlier ones, though he had never perceived it in actual observations. "I have been able to establish the existence of this dead time very easily," he says; "I listened to the second, and when the sound had been perceived I brought my attention to the position occupied by the star."

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[1] Faye illustrates the same theory as follows (Comptes rendus, LIX (1864) 475): "Imagine a moment," says he, "that the mind is an eye placed inside the brain, an eye attentive to the modifications that each sensation determines in the nervous tracts, that end there. If sensations of the same nature are produced at the same point, this inner eye will easily judge whether they are successive or simultaneous; but if they arise from different senses whose nerves end in different regions of the brain, the inner eye will have to move in order to pass from one region to another, and the time thus employed will not be perceived; sensations separated by a very real interval will be noted falsely as simultaneous. The time lost, the time thus employed in going from one sensation to the other, can amount to more than a second. It will vary from one individual to another according to the rapidity with which his inner eye is moved to contemplate successively the keys of that prodigiously complex keyboard that we call the brain.

"I do not need to say that I attach no reality to this comparison; our mind is not an inner eye. Nevertheless, the necessity of comparing two sensations of different origin condemns the mind to a peculiar labor, since it uses a time so considerable to establish a communication between different nervous tracts. This task is also very fatiguing, while the comparison of sensations of the same origin is not so, or is a good deal less so."

[2] AMER. JOUR. PSYCHOLOGY, II, 29.

Of similar import is a series of experiments by Wundt,[1] in which the observer tried to fix the position of a pointer (sweeping over a divided arc) which corresponded to the production of a sound or other stimulus. This is hard to do, and it is necessary to allow the sound to come a number of times in succession before any confident judgment can be made, and even of the results so obtained a very large number are necessary to average out accidental variations. It is easy, for example, to unite the sound with a predetermined position of the pointer, if not too far from the true one; and if all the scale is covered except the single division, the error may amount to a quarter second.

At moderate velocities the sound is regularly associated with too early a position. As the rate of both pointer and recurring sounds is increased, the error lessens and becomes zero—for Wundt himself, when the divisions of the scale are passed over in about $\frac{1}{36}$ second each and the sounds are a second apart. If the rate is still further increased the error changes sign, that is, the sound is connected with a position later than the true one. This, however, cannot go far, because the divisions are soon swept over too rapidly to be distinctly seen.[2]

The error changes in the same way also when, instead of a single sound, a sound and an electric shock, or a sound and another sound of a different kind, are produced together. The two are perceived as simultaneous, but both are associated with a position of the pointer less behind its true position. A third sensation added changes the sign of the error, and a fourth carries it still further in the same direction. The little table below gives the amount of the error (the average at three rates) for four additions; those marked minus are too early, those plus are too late:

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[1] Physiologische Psychologie, II, 334 ff.

[2] Relative personal equations would appear whenever individuals differed in the results obtained in these experiments. One of Wundt's subjects in experiments of this kind always fixed a position of the pointer before the real one.

First added stimulus	—0.0670
Second " "	—0.0113
Third " "	+0.0296
Fourth " "	+0.0399

I pass by developments of these experiments, not immediately concerned with the point in question, to notice Wundt's interpretation of his results. It is something as follows. The division of the scale with which the sound shall be connected depends within certain limits on the adjustment of attention alone,[1] and this is in turn influenced by the rate at which the sounds succeed one another and the pointer moves over the scale. When the rate is such that attention is just at its maximum when the sound is each time produced, there is no error; the sound is associated with the true position of the pointer. When the rate is slower the adjustment takes place too soon and the sound is associated with a position earlier than the true one. When the rate is faster the adjustment is too late and the position later than the true one. The speed at which the pointer moves also influences the rate of adjustment; as the pointer moves fast or slow the adjustment is made rapidly or slowly. When other stimuli are sent in at the same time as the sound, the adjustment becomes more difficult and the time required for it longer; hence the tendency of the first error to grow less and finally to change sign.

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[1] The attention may be thought of as rising rhythmically again and again like a wave, the time at which it reaches its maximum fixing the time at which the sensation to which it is directed will be perceived.

Wundt's "adjustment of attention" is only the psychological explanation of Bessel's indefinite "carrying over of one sensation upon another." Both are agreed in putting the cause of personal equation in the difficulty the mind finds in uniting disparate sensations. C. Wolfe,[1] however, believes that, when such errors as these have been removed by training, there still remains an error due to purely physiological causes. To the examination of this theory and the experiments upon which it rests I turn next.

Wolf clears the ground for the discussion by collecting a large number of personal differences and showing that those larger than 0.3 s are rare. At the beginning of his experiments his own absolute personal equation was of that amount, but as they continued it fell rapidly to 0.11 s, and there remained fairly constant as if it had reached a physiological limit. In the instant of observation an observer does not hear the actual clock-beats, but follows an inner series; he may even go on to complete an observation when the real beats have ceased, without knowing the difference. If this is so, what becomes of the superposition of dissimilar sensations? And again, the seconds need not be taken by ear, but by touch or even by sight, and yet the personal equation remains practically the same.

These last are interesting observations, but it must be perfectly clear that they have no force against Bessel. The illusion of a clock-beat is of its very nature the same to the mind as a real one. When the second is taken by touch there is a dissimilar sensation to be united with that of sight as much as when the second is heard, and even where it is taken by sight there are two things to be attended to, the time and the position of the star, to say nothing of the incipient muscular discharges that probably accompany even subjective counting.

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[1] Annales de l'observatoire de Paris; Mémoires, VIII, 185 ff.

Wolf's theory, however, does not fall with these general arguments; its corner-stone is the following set of experiments. Making use again of his artificial transits, he had, instead of the single little illuminated hole that gave him his star before, three little holes close together in the same vertical line. The upper and lower were illuminated at intervals by induction sparks, the middle one constantly. As long as the plate containing the holes was still, all three seemed, as they really were, in the same vertical line. When the plate was in motion, the constantly illuminated star seemed in advance of the sparks whichever the direction of motion, and further in advance as the rate was more rapid. If the sparks flashed irregularly the star was clear cut; if they flashed regularly, say every second, so that they might be anticipated, the star, though as far ahead as before, had behind it a train extending backward as far as the line connecting the sparks. If the star was made to disappear at the instant the sparks flashed, it was seen as at first on the same line with them.

The interpretation of these experiments is so important to Wolf's theory that I give his own words:

"These experiments seem to me to contain all the facts relative to the personal equation in the case where the second is perceived by sight, and to give the explanation of it. In the first place, the last shows us that the deviation of the star is a pure illusion. The first [*i. e.* that in which the sparks flash irregularly and the star is seen ahead of the sparks without a train] represents very nearly the phenomenon cited by M. Faye to explain the ordinary error of observation. At the unforeseen moment when the sparks flash, the eye, surprised by their apparition, turns its whole attention to them, ceases to see the star (which continues its movement), and does not return to it before the sparks have disappeared. The star has then advanced, and appears in advance by an amount *equivalent to the time during which the luminous impression of the two sparks persists.* There is then, to be sure, between the moment when the second is perceived and the moment when the eye registers the position of the star, a lost time; but in place of attributing, with M. Faye, a purely psychological cause to this lost time, I find for it a physiological origin and a value physiologically determined. The error committed in the estimate of the position of the star is equal to the duration of the persistence of the luminous impression.

"But this case, I have said, is not really that of an experienced observer that perceives the sensation of a perfectly rhythmed second. It is necessary then to go to the second experiment [*i. e.* that in which the sparks flash regularly and the star is seen in advance with a train]. This time the eye, forewarned of the instant of the apparition of the sparks, does not cease to see the star, and at the instant of the explosion sees it on the line of the sparks. But it continues to see them during a certain time during which the star advances and reaches the same position as before at the instant of the disappearance of the sparks. Thus the eye sees the star at once in all the positions comprised between the two extremes that I have just defined. For it these positions are simultaneous; their extent corresponds then to an indivisible space of time. And consequently the observer can relate the position of the star at the moment when the sparks have flashed to any one of the points of that extent.

"But it is necessary to remark besides that at the moment when the eye begins to perceive the sparks, it has still the sensation of all the positions occupied by the star before, during an interval corresponding likewise to the duration of the persistence of the luminous impression. The observer can, therefore, also relate the position of the star at the moment when he perceives the second to any one of the points of this previous extent.

"I will conclude, then, from this analysis, that the personal correction of an observer that perceives by sight an exactly rhythmed second is necessarily comprised between two limits, which are the duration of the persistence of the luminous impression taken positively and negatively."[1]

The next step is to extend the explanation to the case where the time is taken by other senses.

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[1] What he seems to mean can perhaps be shown by the figure below. The lines represent the central star with its train, the dots the occurrence of the sparks. The motion is supposed to be from right to left.

-:   :-

a   *b*

The state of things in experiment two is represented at *a*; any position of the star in the line may be associated with the instant of the sparks. At *b* is represented the state of things in the next to the last paragraph; as before, any position of the star in the line may be associated with the instant of the sparks.

"I will remark," says Wolf:

"1st. That according to the experiments that I have cited above, my personal correction remains the same in whatever manner the perception of the second reaches me, by ear, by sight, or by touch;

"2d. That the duration of the auditory sensation being, according to the physiologists, less than 0.01 s, this duration cannot enter into the cause of personal equation, or at least cannot make it vary except in amounts less than those that we measure. . . .

"It follows from this that the cause of the personal error ought to be here the same as in the first case, that is to say, it is still found in the combination of the persistence of the luminous impression with the continuous movement of the star."

The author then repeats in substance the explanation of the positive and negative limits given above, and restates the conclusion for the case of eye and ear observations.

Wolf shows, earlier in the paper, that when the apparent motion of the star is suppressed by illuminating it with a spark from second to second, the personal equation practically disappears, and now adds a last item of evidence in showing that the duration of the after-image under conditions like those of his experiments was from 0.05 s, when the stimuli fell continuously on one point of the retina, to 0.16 s, when the image moved upon the retina. The absolute personal equation, of the kind in question, therefore should not exceed 0.16 s. In case the eye is not stationary (as has been supposed in the discussion, and as Wolf believed his own to be during the time that the star was near the wire) the explanation is still applicable; for if the eye followed the star, the images of the wires would move upon the retina, and the after-sensations left by them would bring about the error.

I have quoted from Wolf's interpretation above because I propose to criticise it; I pass by minor matters for his cardinal experiment, experiment two. Four things contribute to this illusion: (1) the persistence of the images of the sparks; (2) the persistence of the image of the star; (3) its motion, and (4) the turning of a portion of the attention to the sparks. Assuming Wolf to be correct in implying that the attention or a part of it is turned to the sparks and remains there till the after-images have disappeared, it is clear that if the images of the sparks had no persistence, the middle point could not move during their persistence, nor be seen in advance of them; its train therefore depends on their persistence. In his further discussion Wolf seems to forget this essential point and speaks only of the train, that is, of persistence of the star's after-image. This omission lets in two errors. In the first place, if the train of star after-images depends on the persistence of the after-images of the sparks, there can be no train before the sparks flash, and consequently no associating the flash with a position of the star behind the true one, that is, no minus personal corrections. And second, if the persistence of the spark-images is essential to the illusion, it is wrong to think that the illusion is the same when the sparks are replaced by a sound of which the after-sensation is confessedly less than 0.01 s. It is, of course, easy to avoid these difficulties by making the time given to the sparks due to the inertia of attention and not to the persistence of their after-images, but that would be to surrender the point for which Wolf is contending. At the same time it is not to be denied *a priori* that physiological inertia may enter to a certain extent. The fact that the eye and ear personal equation is larger for bright stars than for faint ones, as proved by Bakhuyzen,[1] could be cited in support of such a view. On the whole it may be admitted that Wolf has brought to notice a possible factor in personal equation, but it cannot be admitted that he has demonstrated the large place for it that he claimed.

Since the beginning of the year, another physiological explanation has been proposed by J. J. Landerer.[2] He finds that with very many people, himself included, there is a monocular doubling of the image of a single bright point seen at the distance of distinct vision or a little beyond, the second image being a little fainter than the original. This doubling in many is from left to right in the right eye, and from right to left in the left eye, though in some it is vertical or inclines, and in one individual was not found at all. Even when observed with a lens, the double partly overlies the principal image. In view of this, he believes "that the observation of a little luminous disk is made not on its center, but to the right or left, above or below, according to the direction of the diplopia, the eye quite naturally having regard to the center of the group that the two images form. It follows then that the efficient cause of personal equation properly so called rests in this physiological effect, or at least that this plays a preponderant role." If this is so, it is easy to calculate the personal equation for different powers of eye-piece and different declinations, and in the author's case the values so found agree within a few hundredths of a second with those found in the ordinary way.

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[1] See AMERICAN JOURNAL OF PSYCHOLOGY, II, 278.

[2] Comptes rendus, CVIII, 219; Feb. 4, 1889.

It is impossible to deny off-hand that such optical irregularities do affect observation, but after all that has been adduced in evidence of the general complexity of personal equation and the probable importance of the psychic factor in it, I cannot agree in the important rank that the author assigns to them. There is also some reason to think that some at least (in using the chronographic method) observe rather the advancing limb of the star's image than the center.

Bredichin[1] incidentally suggests a special illusion in eye and ear observations—a *sui generis* persistence of apparent motion, arising from the constant decreasing of the distance between the star and wire before the transit and the constant increasing of the distance after the transit. This would of course only fit the case of those that observe too early. Such a thing is an interesting possibility, but so far as I know there is as yet no evidence for it, except so far as it might rest on the play of attention already many times mentioned.

APPENDIX.

I add this appendix chiefly for two reasons. The first is that I wish to thank Professors Eastman and Winlock and Mr. A. S. Flint of the Naval Observatory for courtesies shown me at that institution. The second is that I may give what bibliographical references I have gathered. The list is not exhaustive. I have rather set down such references as it has seemed to me, either from having seen the articles themselves or from some other reason, might be convenient in preparing such a general sketch as has been presented. As that was confined to the form of personal equation that attends time observations, so no references are here given to any other form. The list can be enlarged somewhat, if any one desires, from Houzeau and Lancaster's *Bibliographie générale de l'Astronomie*, Vol. II, 902 ff. and

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[1] Annales de l'observatoire de Moscou, II, 2nd pt., p. 69.

1685, and from Sinclair's *General Index of Scientific Papers contained in the Appendices of the Reports of the U. S. Coast Survey:* Report for 1881, app. 6, pp. 91-123. Figures for the relative personal equation, at least, can also be found in the reports of almost every determination of longitude and in the transit observations of the different observatories.

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MEMORY, HISTORICALLY AND EXPERIMENTALLY CONSIDERED.

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III.

PARAMNESIA.

Having brought this study of memory down to recent times, I propose, in the present section, to turn aside somewhat from strictly historical and experimental lines, to consider a class of phenomena long known, yet but little investigated. I refer to pseudo-reminiscences or the phenomena of paramnesia.[1] The importance of the subject justifies the digression.

While amnesia, including aphasia in its various forms, has long been a subject of scientific observation and study, and while the more remarkable phenomena of hypermnesia have received some consideration, the subject of paramnesia has until recently been neglected. Even Ribot in his excellent monograph on "The Diseases of Memory" considers only one form of it, *i. e.* the so-called "double memory," or feeling in a new place of having been there before. This he mentions as of rare occurrence, and says that only three or four cases are on record.[2] Sully, however, in his work on "Illusions" devotes a chapter to the subject;[1] and recently Dr. Emil Kraepelin has made an elaborate study of the more remarkable forms of paramnesia.[2]

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[1] I shall use the word paramnesia, suggested by Kraepelin and formed after the analogy of paranoia, paraphasia and the like, as a general term to denote pseudo-reminiscences or illusions and hallucinations of memory.

[2] See "The Diseases of Memory," New York, 1882, p. 186.

Many writers, however, have mentioned illusions of memory. St. Augustine, so far as I know, was the first to allude to them.[3] He refers to their occurrence in dreams as a matter of common observation; and it is noteworthy that he anticipated the suggestion of Bastian and others, that pseudo-reminiscences give rise to the belief in metempsychosis.[4] Among the earlier psychologists of modern times, Hume and Hartley refer to illusions of memory as not uncommon, especially among liars.[5] More recently Feuchtersleben[6] mentions pseudo-reminiscences, calling them "phantasms of memory"; Jessen[7] refers to them, and Carpenter[8] gives some examples as "fallacies of memory." In general literature, also, there are occasional allusions to the phenomena.[9]

Among recent English writers Sully[10] has treated the subject more at length than any one else. He divides illusions of memory into three classes according to the three things involved in a complete act of memory (we remember that a thing happened and how and when). "Thus we have (1) false recollections, to which there correspond no real events of personal history; (2) others which misrepresent the manner of happening of the events; and (3) others which falsify the date of the events remembered."[1] In his opinion these illusions of memory correspond to visual illusions. Class one is analogous to the optical illusions called ocular spectra. Such mnemonic errors may be called hallucinations of memory. Class two is like those optical illusions where a real object is seen, but its image is distorted by the refracting media between the object and the eye. Class three corresponds to erroneous perceptions of distance due to the clearness of the atmosphere and the absence of intervening objects.

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[1] Illusions, Ch. X, New York, 1881.

[2] Ueber Erinnerungsfälschungen, Archiv f. Psychiatrie, Bd. XVII, H. 3, and Bd. XVIII, H. 1 and 2.

[3] See my first article, this Journal, Vol. II, p. 61.

[4] Comp. Berliner Medicinisch-Psych. Gesell.; Sitzung vom 19 Dec. 1871, Archiv f. Psych. III, p. 505; also Emminghaus, Allgem. Psychopathologie, p. 132.

[5] See my first article, pp. 81 and 84.

[6] See his Principles of Medical Psych., Eng. translation, London, 1847, p. 238.

[7] See his Versuch einer wissensch. Psych. (mentioned by Kraepelin, Archiv f. Psych., Bd. XVIII, p. 409).

[8] Mental Phys., 5th ed., p. 456 seq. See also an article by Frances Power Cobbe on the same subject, Galaxy, 1866.

[9] See infra, pp. 439, 448.

[10] Illusions, Ch. X, New York, 1881.

Kraepelin, who has devoted his study to Sully's first class—the "hallucinations of memory"—divides these pseudo-reminiscences themselves into three classes, as follows: (1) simple pseudo-reminiscences (*einfache Erinnerungsfälschungen*), where the images of the imagination as they arise spontaneously in the mind appear as reminiscences; (2) associating pseudo-reminiscences (*associirende Erinnerungsfälschungen*), where a present perception calls up by association pseudo-reminiscences of something analogous or related in the past; (3) identifying pseudo-reminiscences (*identificirende Erinnerungsfälschungen*), where a new experience appears as a photographic copy of a former one. This division is somewhat arbitrary, and it is not always clear in which class a given case belongs; but until more cases have been observed, and further study has been made, this division may be useful as a provisional classification.[1] The cases reported since Kraepelin's articles appeared may be arranged under his rubrics, and I shall adopt them in this article, though with a change of order and terminology.

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[1] Loc. cit., p. 243.

I.—*Simple Paramnesia.*

In this form of paramnesia, the images of the imagination, as they spontaneously arise in consciousness, appear as memories. Even among normal individuals, as Kraepelin has said, pure inventions of the fancy may assume the aspect of reminiscences. This is especially noticeable among children and aged people. If complete scenes and stories are not manufactured by the imagination and made to counterfeit true memories, yet details are filled in and related with evident sincerity. More commonly, these figments of the imagination do not at first appear as things remembered, but gradually by repetition the pseudo-reminiscence is developed. This is notably the case with liars. It has often been pointed out that after a time they are liable to believe their own stories. Many narratives of adventure, of haunted houses and the like, told first for the sake of amusement and with knowledge that they were false, may have thus gradually come to be believed.

The ordinary defects of memory are well known. Sully has called attention to the fact that the common phrases of daily life—such as, "Unless my memory plays me false," and the positive assertion of one whose memory is doubted, "I know that I remember this, or else I dreamed it"—indicate a universal suspicion that memory is not quite trustworthy. Those whose special work is to weigh evidence are apt to put still less confidence in memory unsupported by documentary evidence. Lawyers are proverbially suspicious of their own memories, as well as those of others; and historians have to depend chiefly upon written testimony.[1]

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[1] Kraepelin indeed offers this only as a provisional classification; v. op. cit., pp. 435, 436.

While, apart from the cases already mentioned, normal individuals seldom or never project a whole series of imagined events into the past as memory pictures, the imagination often supplies the separate members of a remembered series. We remember certain points, the imagination fills out the picture. The following report given me by one of my students will illustrate my meaning:

> "During the summer of 1888 I was requested to testify before a notary public regarding the details of an accident which I had witnessed two years previous. A large crowd had gathered in front of a prominent citizen's house to witness a display of fireworks on the fourth of July—among the rest my friend (the plaintiff in the case) and myself. My friend was struck in the eye with a ball of fire from a Roman candle. In testifying before the notary I swore that the ball of fire *had emerged from the tree where it had lodged after being discharged, and from there fell towards the earth, striking the plaintiff.* This was my impression, and I had not the slightest doubt at the time but that my statement was absolutely correct. Several other witnesses of the accident were present in the room while I was testifying, and after the testimony had been taken I found that the unanimous opinion of all the other witnesses was *that the fire-ball*

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[1] Col. Nicolay is reported to have said that he and Mr. Hay received very little aid from contemporary memories in writing their history of Abraham Lincoln, and that they came to the conclusion that mere memory unassisted by documentary evidence was "utterly unreliable after a lapse of fifteen years."

did not emerge from the tree at all, but came directly from the hands of the discharger. I was much chagrined at the unanimous protest raised by the other witnesses, and had I been asked again three minutes afterwards I should have said that I could not say whether it came from the tree or not. As far as my own memory serves me I cannot say even to-day whether the ball of fire came in one way or the other. In consideration of many facts, however, I have concluded that I was mistaken."

The way the pictures of the imagination may counterfeit true memory-images will probably appear to nearly every one that is able to observe his own dreams. In dream-life, new scenes and faces, the creations of unbridled fancy, appear familiar to us. The distinction between the images of imagination and those of the memory—always indefinite—is almost obliterated. The images of the former masquerade in the dress of the latter. At least so it seems when we look back upon our dreams from waking life.[1]

Kraepelin relates the following instance from his own experience. Though never having smoked in his life, he dreamed of smoking a cigar, had clearly the taste of tobacco, and said in his dream *optima fide* that he was smoking his fourth or fifth cigar. A number of dreams containing similar pseudo-reminiscences have been reported to me, and I have noticed several among my own. One of the most noteworthy I have already reported in this Journal.[2]

This form of paramnesia is very common among the insane. Kraepelin, while observing it in other forms of insanity—as melancholia and mania—found it with especial frequency in *dementia paralytica*. Here the pseudo-reminiscences are very closely connected with great ideas of the present. The patient relates a mass of adventures—great journeys, meetings with noted persons, and the like. It is often hard to tell whether one is dealing with mere bragging or really with paramnesia. For a time the patient may have a vague feeling that what comes into his consciousness in this way is false, just as he often feels somewhat insecure at first about his great ideas; but, finally, doubt recedes, and the pseudo-reminiscences are assimilated as the morbid fancy presents them.

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[1] Of course, if one cares to argue that what seem memory-images are really what is remembered from former dreams, as in the hypnotic trance one remembers what has occurred in former hypnotic states, it will be as hard to refute him as it will be for him to prove his case.

[2] Vol. I, p. 735.

"As a separate feature," says Kraepelin, "the trouble here described is found very often in the diagnosis of paralysis, although it has hitherto been scarcely considered. A patient in Munich maintained almost daily that his wife had visited him and made him presents; another, whom I recently saw, related that the shoemaker had been there the day before and had brought clothes with him and money, also hats—thirty-five hats and a piece of gold. A third, on his entrance into the asylum, said: 'To-day is the funeral of our people; they have all shot themselves this week.' A female patient in Leubus declared: 'The woman (the attendant) sent mother coffee and bread yesterday. She is lying right over there.' In a short time, with a little attention, a great number of similar remarks can easily be collected.

"On the other hand, in contrast with this more sporadic appearance of the pseudo-reminiscence, there are cases in which the whole thought and action of the patients is greatly influenced by these morbid troubles. They no longer have any idea of what has actually happened, but reconstruct the whole course of the day, even to details, out of pseudo-reminiscences dovetailed together."[1]

This form of paramnesia is of special interest in some cases of melancholia. The pseudo-reminiscences appear at first, perhaps, as imperative ideas, and the patient strives against them; but after a time they become established as remembered events. Many self-accusations probably begin in this way. An interesting case reported by Kraepelin may be mentioned in illustration, though the patient does not appear to have been a melancholiac. The patient related in detail and with complete conviction how she had such a delight in evil that she had destroyed a great number of wills that had accidentally fallen into her hands. Moreover, she declared that in her youth she had "numberless times" put pins into bottles of medicine that others might swallow them.[2]

It is often very difficult to distinguish a genuine pseudo-reminiscence. What appears as such may often be the memory of something dreamed or read, or the reminiscence of an hallucination of sense. The patient's lively imagination and weak judgment make it impossible for him to decide what has actually happened. A general test for determining whether one has a genuine case of simple paramnesia or the reminiscence of an hallucination of sense, a dream, or the like, has been given by Kraepelin. In the former case the train of ideas is variable—the patient seldom repeats the same story; in the latter case the essential features of the reminiscence remain constant.

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[1] Kraepelin. Loc. cit., pp. 215, 216.

[2] Loc. cit., p. 211, foot-note.

II.—*Identifying Paramnesia.*

This form of paramnesia is very common. It is the so-called "double memory," or "been-here-before" feeling of daily life. Perhaps half the people one meets can tell of some experience where in new surroundings they have had the perplexing impression of having been there before. Allusions to such an experience are frequent in literature. Coleridge, Rossetti, Hawthorne, Zschocke, Dickens, and others have referred to it.[1]

So far as I am aware, the only systematic enquiry that has been made in reference to this phenomenon among normal persons was undertaken a few years ago by Prof. Osborn.[2] He distributed at Princeton and elsewhere the following question in connection with Mr. Galton's series of questions on visualization: "Have you come suddenly upon an entirely new scene, and while certain of its novelty, felt inwardly that you had seen it before, with a conviction that you were revisiting a dimly familiar locality?" This report of Mr. Osborn's study is, unfortunately, a popular one. While he refers to his correspondence as "extended," he neglects to report the number of his correspondents, or what proportion of the whole number questioned answered in some way. He reports, however, that "this question elicited affirmative replies from about one half the correspondents, covering experiences of considerable variety."[1] One of the cases that he gives as representative was reported to him in the following words:

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[1] Hughlings Jackson cites Dickens, who describes this common experience as follows: "We have all some experience of a feeling which comes over us occasionally, of what we are saying and doing having been said or done before in a remote time, of our having been surrounded dim ages ago by the same faces, objects, and circumstances, of our knowing perfectly what will be said next, as if we suddenly remembered it."

[2] See Illusions of Memory, North American Review, May, 1884, p. 476 seq.

> "Somewhat more than a year ago, I visited the city prison of Mazatlan, in Mexico. It consisted of a court open to the sky, on three sides of which the cells opened, the fourth being a high wall. The entrance was by an arched passage-way, with three barred gates. The court paved with cobbles, the entrance, the several rooms, every surrounding internally, seemed as familiar to me as home. Not so with any portion of the exterior. Yet I had never been within three thousand miles of the place until this journey."

Most of the cases of this form of paramnesia so far observed seem to have been of normal individuals. I do not feel sure, however, that Kraepelin is right in saying that it "belongs almost exclusively to normal life." Further observation may show that it occurs as frequently, or even more frequently, in pathological cases. Certainly it is not uncommon among the epileptic;[2] and the cases already reported[3] show that it also occurs in other forms of insanity, as will appear in the following pages.

The first pathological cases reported, so far as I know, were observed by Neumann.[4] He looked upon the phenomenon as a sort of mental mirage, and called it *Empfindungsspiegelung*. Of the two cases observed by him, one was an adolescent; the other an epileptic woman. The former when brought to the asylum maintained that he had been there before, that he had talked with the physicians, that the same things had been said to him, that he had before had the same room, that he had eaten the same food, etc. In case of the latter, the paramnesia became so extreme that, when reading history, the patient thought that she herself had experienced the events related. In both these cases the illusions caused excitement and anxiety. Afterwards cases were reported by Jensen[1] and Sander;[2] and one of the most remarkable cases on record was observed by Dr. Pick.[3] The following account condensed from the original report will give the most interesting features of the last mentioned case:

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[1] Loc. cit., p. 478.

[2] See Hughlings Jackson: "Intellectual Aura," Brain, July, 1888. See also infra, p. 442 seq.

[3] These cases are mentioned by Kraepelin, loc. cit., p. 428. See also the Autobiography of a Paranoiac, by Dr. Peterson, this Journal, Vol. II, p. 198; other doubtful cases have been reported by Jensen and others.

[4] Lehrbuch d. Psych., pp. 111, 112.

> The patient was a young man of considerable intelligence and of a very good memory. His illusions began in a marked degree when he was about 23 years of age. As the psychosis developed, he had delusions of persecution and aural hallucinations, and was brought to the asylum. In the diary that the patient kept he reported that, on the second day of his stay in that institution, it seemed to him that he had been there before. The patient kept quiet most of the time, and seldom spoke, except of his illusions of memory and the insane ideas connected with them. From reflection on his illusions the patient concluded that he must have a double life, made up of recurring periods of similar events.
>
> The chronic form that the false memory assumed will appear from the following account given by the patient: "The first clear experiences of a double life I had in the autumn of 1868 at St. Petersburg. But these occurred only occasionally; for example, on visiting places of amusement, or at great festivals, and when meeting persons, the accompanying circum-

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[1] Allgem. Zeitschr. f. Psych., Bd. XXV, Supplement Heft, S. 57 seq.

[2] Archiv f. Psych., Bd. IV, S. 244 seq.

[3] Archiv f. Psych., Bd. VI, S. 568.

stances seemed so familiar to me that I firmly believed that I had already been in the same place and had met the same persons under just the same circumstances, at the same season of the year, in the same weather, the men standing in the same places, in just the same manner, and even precisely the same conversation occurring. . . . After 1870 almost every piece of work that I attempted in my business seemed familiar to me, as if I had already done the same in former years, in the same order and under exactly the same circumstances; not only this, but even every chance meeting with any one, and in general everything that occurred around me, brought this feeling. It came to me sometimes at the moment of perceiving a thing, or after some minutes or hours, frequently not until the next day."

A similar case is reported by Forel.[1]

The patient was a young merchant, of good education, like Pick's patient, brought to the asylum with delusions of persecution. On entering the asylum he maintained that he had been there a year before, and he recognized everything in the manner described above. From these false memories the patient got the idea that he had actually lived through his present experience before under similar circumstances, and that on occasion of his former entrance into the asylum and dismission from it he had been stupefied, and thus robbed of the immediate recollection of those events. Now as he saw and experienced all these things again, he began to recall them. Hence the patient believed that he was a year further on in time, and he persistently wrote 1880 instead of 1879. To corroborate his memory he was ready to refer to the asylum reports of the previous winter. This patient's paramnesia continued after he left the asylum.

Kraepelin cites two cases of epilepsy where the patient had this form of paramnesia.[2] Hughlings

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[1] Cited by Kraepelin, loc. cit., p. 430 seq.

[2] Loc. cit., pp. 428, 429. One of Neumann's cases was an epileptic. See his Lehrbuch der Psychiatrie, p. 112. See also Jensen's report, loc. cit., p. 57; and Sander, loc. cit., p. 252.

Jackson[1] has reported several cases where, in the "intellectual aura" or "dreamy state," false memories occurred. One of the most important of his cases is that of a highly educated physician who is subject to attacks of *petit mal* and *haut mal*. In his report of his own case this gentleman mentions illusions of memory in the initial stages both of *petits maux* and *hauts maux*. Speaking of his mental condition in the former, he says:

> "In a large majority of cases the central feature has been mental and has been a feeling of recollection, *i. e.* of realizing that what is occupying the attention is what has occupied it before, and indeed has been familiar but has been for a time forgotten, and now is recovered with a slight sense of satisfaction, as if it had been sought for. My normal memory is bad, and a similar but much fainter feeling of sudden recollection of a forgotten fact is familiar. But in the abnormal states the recollection is much more instantaneous, much more absorbing, more vivid, and for the moment more satisfactory, as filling up a void which I imagine at the time I had previously in vain sought to fill. At the same time, or perhaps I should say more accurately, in immediate sequence, I am dimly aware that the recollection is fictitious and my state abnormal."[2]

In another case reported by Ferrier a woman had attacks of *le petit mal* that were divided into three distinct stages, of which "the first stage is a dreamy state or reminiscence, in which everything around her seems familiar or to have happened before."[3]

Several years ago, another physician, subject to attacks of epilepsy, suggested that this form of paramnesia might serve as prognostic of epilepsy. In his own case he came to treat the experience "as an indication for immediate rest and treatment." Apropos of this case Hughlings Jackson says, "I should never, in spite of Quaerens' case, diagnose epilepsy from the paroxysmal occurrence of 'reminiscence' without other symptoms, although I should suspect epilepsy if that super-positive mental state began to occur very frequently, and should treat the patient according to these suspicions were I consulted for it."[1] He emphasizes, however, the advantage of noting this phenomenon as a possible symptom of epilepsy.

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[1] Loc. cit.

[2] Loc. cit., p. 202. This is not clearly a case of this form of paramnesia, but is interesting on account of its connection with epilepsy.

[3] Cited by Jackson, loc. cit., p. 198.

Similar phenomena seem to occur frequently in dreams. Probably almost every one will find instances by noticing his own for a time. Several cases have been reported to me. The following, given me by one of my students, will serve as an illustration:

> "I remember that once in a dream I entered a second-hand book-store. The place was perfectly familiar to me, and I spent some time there looking at books and talking to persons I knew. On awaking I knew that I had never been in any such store. In the dream I did not recognize it as a *new place* that I was remembering, but only on waking."[2]

An Italian psychologist[3] says that he has often observed this form of paramnesia in himself, and he reports a dream containing a similar illusion. The following is the substance of his report:

> "Last night I dreamed of having occupied with my family a portion of a certain house situated in some city. While discussing with my wife the arrangement of the furniture and the use of the different rooms, I remembered with perfect clearness having lived in the

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[1] Loc. cit., p. 186.

[2] Comp. a case reported by Kraepelin, loc. cit., p. 413.

[3] Francesco Bonatelli: Il fenomeno della ricordanza illusoria. Rendiconti della Reale Accademia dei Lincei, Vol. IV, fasc. 4.

> same apartment several years before, and spoke of the arrangement of our furniture at that time. The dream was so impressive that upon waking I had at first no doubt that I was recalling what had actually happened. On reflection, however, I was convinced that my recollection was false."

The writer seems more than once to have dreamed of this apartment, and to have had similar illusions of memory.

By considering the cases cited it will be seen that they differ in one important point. In case of the normal persons the illusion is immediately corrected (although it is possible that other cases occur where the illusion is not corrected and hence not observed). In clearly pathological cases like those reported by Pick and Forel, and also in dreams, the illusion is not corrected. This form of paramnesia seems to occur most frequently among young people and those gifted with vivid imagination,[1] and at times of fatigue or excitement.[2]

Kraepelin distinguishes the following characteristics of this form of paramnesia.[3] In the first place, the whole picture of the present moment is recognized as an accurate copy of the supposed original in the past. In most cases the illusion appears suddenly and vanishes suddenly. There is a vague feeling that one knows what is to happen next. Finally, there is often a vague feeling of uneasiness or anxiety, due to the vain attempt to comprehend clearly the obscure ideas floating before the mind.

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[1] This is the opinion of Kraepelin, loc. cit., p. 410, and the cases I have studied corroborate this view. The number of cases reported, however, is too small to warrant a positive statement.

[2] Anjel, Archiv f. Psych., Bd. VIII, p. 57 seq. See also Bonatelli, op. cit.

[3] P. 424 seq. Sander mentions substantially the same characteristics, loc. cit.

Many hypotheses have been invented to account for this phenomenon. Wigan noticed this form of paramnesia in himself, and explained it in accordance with his theory of the dual nature of the brain. In his opinion, on occasion of such an illusion, at first one hemisphere functions alone, then the other wakes up, and to their fused consciousness the vague impression of the former is a recollection. As the original impression was too indistinct to be fairly fixed in memory, we have no means of localizing it definitely in the past; thus it may seem to have occurred years before. Jensen[1] and Wiedemeister[2] have based their explanation upon the same theory.[3] Anjel[4] has propounded the ingenious theory that in such illusions the processes of sensation and perception which usually overlap become separated. The mind, on account of fatigue, is unable to perceive (or, to use the modern term, to apperceive) the sensation when it occurs; and, when the tardy process of perception does occur, the mind is unable to distinguish this fading sensation from a reproduced impression. In proof of this he states that he has always found that these illusions occur when one is fatigued or when the attention is momentarily distracted. He cites the case of a lawyer who, in the strain of a difficult lawsuit, was suddenly seized with this form of paramnesia. Here the illusion was the premonition of nervous disease. Anjel noticed similar effects of fatigue in his own experience. After spending hours in the Venetian art galleries, he suddenly felt that he had already seen the paintings before him, although he knew that this was impossible.[1]

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[1] See loc. cit., also Archiv f. Psych., Bd. IV, S. 547 seq., where he cites in support of this view the case of a patient subject to attacks of migraine in the left side of the head. The patient had this form of illusion in the state preceding an attack.

[2] Allgem. Zeitschr. f. Psych. Bd. XXVII, p. 711 seq. He calls this phenomenon, inaptly enough, "double consciousness."

[3] Dr. Maudsley also favors this theory. See The Double Brain, Mind, Vol. XIV (No. 54), p. 187.

[4] Loc. cit.

In opposition to these theories, Jessen,[2] Sander,[3] and others have held that there is a vague recollection of some kind, upon which the pseudo-reminiscence is based. According to Sander, this phenomenon is a result of false association. Some similar scene has been witnessed, imagined, or dreamed in the past, and part of the elements of the situation are truly remembered.[4] Sully[5] and Buccola[6] also have maintained that the illusion often arises from remembering the events of dreams and localizing them in our waking life. Emminghaus thinks that we identify a present situation with a former one in some respects similar, because, owing to a too rapid flux of thought, we apperceive the points of resemblance and neglect the differences.

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[1] A fellow-student of psychology suggests a theory that is quite the obverse of this. He has often observed this form of illusion in his own dreams, and thinks they generally occur in morning dreams. The over-rested condition of the nerve centers may, he thinks, explain this phenomenon. When we see a strange object, its unfamiliar aspect is largely due to the difficulty we find in apperceiving its characteristics. The process of becoming acquainted with a thing consists in making the act of apperception easy. Hence, when the brain centers are over-rested, the apperception of a strange scene may be so easy that the aspect of the scene will be familiar. The fact observed by Anjel that this illusion is apt to occur in conditions of fatigue does not necessarily conflict with this explanation. In the cases observed there may have been an abnormal ease of apperception due to hyperaesthesia induced by the fatigue. It may be added that Bonatelli thinks that illusions of memory occur in states of unusual nervous irritability. Such, in his opinion, would be the condition in vivid dreams and in the unusual circumstances of journeys and the like. For his own theory see op. cit.

[2] Op. cit.

[3] Loc. cit., p. 252.

[4] Sander admits, however, that this explanation will not suffice in all cases.

[5] Op. cit., p. 274.

[6] Le illusioni della memoria. Rivista di filosofia scientifica, 1883, II, pp. 2, 6. Cited by Kraepelin.

Kraepelin maintains that this theory of a basis of true recollection does not solve all the difficulties, and that it can only claim to be a hypothesis. He rather favors the view that at least in such cases as those reported by Pick and Forel, the false recollections are created out of whole cloth by the imagination.[1]

Cases enough have not yet been reported to furnish data for a satisfactory hypothesis. It may, however, be safely said that in most cases among normal individuals the false recollections are distorted memories of actual events. They are illusions, not hallucinations. We have seen or imagined something similar. That Buccola's explanation may often be the correct one is made probable by the observations of Radestock and others.[2]

Hawthorne has given, in "Our Old Home," an excellent illustration of the way a vivid imagination may furnish material for an illusion of this kind.[3]

> After describing his visit to Stanton Harcourt, he gives an account of the old kitchen of the castle, with its huge fireplaces, blackened walls, and conical roof seventy feet above the hearth, and adds: "Now—the place being without a parallel in England, and therefore necessarily beyond the experience of an American—it is somewhat remarkable that, while we stood gazing at this kitchen, I was haunted and perplexed by

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[1] Op. cit., p. 424 seq.

[2] Radestock in *Schlaf und Traum* relates that when keeping a record of his dreams he often felt that he had seen something before without being able to tell when or where. On turning to his record, he generally found that he had dreamed something similar. One of my students reports a similar experience. See also Carpenter, Mental Phys., 5th ed., p. 456.

[3] This instance apparently lacks some of the elements of complete identification upon which Kraepelin insists, but it is nevertheless an instructive case.

an idea that somewhere or other I had seen just this strange spectacle before. The height, the blackness, the dismal void before my eyes, seemed as familiar as the decorous neatness of my grandmother's kitchen."[1]

Hawthorne himself discovered the cause of his illusion, and reports it as follows: "Though the explanation of the mystery did not for some time occur to me, I may as well conclude the matter here. In a letter of Pope's, addressed to the Duke of Buckingham, there is an account of Stanton Harcourt. . . . It is one of the most admirable pieces of description in the language, playful and picturesque, with fine touches of humorous pathos, and conveys as perfect a picture as was ever drawn of a decayed English country-house; and among other rooms, most of which have since crumbled down and disappeared, he dashes off the grim aspect of this kitchen. . . . This letter, and others relative to his abode here, were very familiar to my earlier reading, and remaining still fresh at the bottom of my memory, caused the weird and ghostly sensation that came over me on beholding the real spectacle that had formerly been made so vivid to my imagination."[2]

III.—*Suggested or Associating Paramnesia.*

In this form of paramnesia the pseudo-reminiscence is analogous or related to a present experience. An actual impression suggests an illusion or an hallucination of memory.

Mere illusions of memory suggested by present impressions are common in normal life. As we apperceive any object or event through the media of the feelings and ideas in consciousness at the moment, and thus no two of us apperceive the same thing in the same way, so in recollection each apperceives the past from the standpoint of his present state of consciousness, and the latter bears its part in determining what the resulting recollections shall be. We remember only main features of an event anyway, and the imagination fills in the gaps. Thus remembrance is never a true reproduction of reality. It is always more or less an illusion. At best it is an approximation to the truth. How near an approximation depends largely upon the apperceptive mood of the moment.

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[1] Our Old Home, p. 213.

[2] Op. cit., pp. 213, 214.

Among the insane who are subject to this form of paramnesia, any present impression may be the impulse to a pseudo-reminiscence. For example, when one of Kraepelin's patients met any one, he was liable to have a pseudo-reminiscence of having done something or having been somewhere with that person.

Kraepelin has called attention to the necessity of distinguishing between pseudo-reminiscences and certain illusions due to defective observation.[1] A patient may call a person by a wrong name, either on account of a pseudo-reminiscence connected with the person, or because of false perception due to defective vision, inner voices, revelations and the like. In the former case the memory-picture is distorted to resemble the present impression. In the latter, the present impression is remodeled to fit the memory-image.

The more extreme forms of suggested paramnesia, such as may be called hallucinations of memory, possibly occur sporadically among normal individuals. No very satisfactory cases, however, have yet been reported.[2] In dreams, however, they are probably not uncommon. I take a simple instance from my own experience. The dream occurred years ago before I had anything to do with psychology, yet it made such an impression upon my mind that I have never forgotten it. I dreamed of receiving a postal-card, and at once remembered writing a letter, to which the card before me was an answer. Upon awaking I knew that I had never written such a letter. Here was a pseudo-reminiscence suggested by a present impression, but with apparently no basis whatever in fact. Two cases have been reported to me by a friend, himself a psychologist. The first dream contains a simple pseudo-reminiscence as well as one of the class under consideration. The part of the dream relating to the latter is reported by him as follows :

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[1] Loc. cit., p. 230 seq.

[2] But see infra, pp. 462–463.

> "As I went down a street in Baltimore (not, I think, any real one), I had a distinct recollection of being told by Mr. C. that this was one of the aristocratic streets in old times." While the memory of a portion of the dream was somewhat vague, my correspondent felt "clear and certain" upon the point in question. "I distinctly remember the thought," he says, "'This is the street I have been told about,' while I was really told about none. I even believe now, though I naturally cannot be absolutely certain, that this thought was accompanied by a mental picture of the telling, such as I sometimes experience when I remember such things very vividly."

The notes on the second dream were made immediately upon waking, and the full report written up in a few hours. It likewise contains two pseudo-reminiscences ; I quote, however, only the portion of the report that concerns us here.

> "In my dream I enter a room and see at the supper-table two young fellows. One of them tells me that Mr. B. has been arrested at the South (Ky.), and been refused extradition from the State because he has at one time had a 'warrant' served on him here for challenging a man's vote.[1] On hearing this, I have the

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[1] My correspondent's legal knowledge in dreams seems to be as erratic as his memory.

recollection quite clearly of having heard the same story (at which of course I was much surprised) some time before from Mr. B. himself. The only event in waking life having any relation to this pseudo-reminiscence is, that Mr. B. actually did tell me not long since of a strange law in Virginia or North Carolina; nothing, however, having any relation to the subject-matter of my dream."

A similar dream is reported by one of my students.

In his dream he ascended a mountain in the Blue Ridge. "From this place," he continues, "I distinctly saw the farm-house where I am accustomed to stay, exactly as it actually exists. In one of the fields I thought carpenters were at work preparing fair-grounds, all the details of which I can remember—race-track, flying-horses, etc. I thought then that I remembered having been told that that field was to be sold for just such a purpose. In reality nothing of the kind is the case. My friend never had an idea, that I know of, of selling one of his fields, much less selling one for a fair-ground."

The pseudo-reminiscences in the following dream, reported by the American Society of Psychical Research, are closely related to the class under consideration:

"I will tell you a *dream* which I had a few nights ago, which possibly may be of interest in connection with these questions. My friend, C. W. B., visited us recently, and spoke with Mrs. A. and me repeatedly about his several trips to Europe, describing especially his experiences in Spain during his last trip.

"A few nights later, I dreamed of looking over with him a lot of large photographs of scenes in Scotland, which he took when we were in Scotland together; many of the photographs showing me very plainly in various attitudes with different groups of people. Now, Mr. B. and I were never in Europe together, and I was never in Scotland in my life. Yet as each photograph was shown, I felt all the keen delight of recognition of well-remembered scenes, and frequently exclaimed, 'How well I remember that!' or 'Don't you remember the day we were there?' etc. I can still remember the features of several of the pictures, parks, grounds, etc., as they appeared in these photographs, and my keen interest in seeing them *again*, and my memory of many incidents and particulars of our being at these places together at some former time. I then dreamed, with the well-known inconsistency of a dream, that in the case of one place Mrs. A. had been with me, and I turned and asked her if she did not remember the day we were there, and what the old lady in charge of the place had said to us.

"If I could in this dream have so strong a sense of having been in the photographed places before that each brought up a flood of remembered experiences, all of which were—pictures and remembrances—the coinage of the dream at that moment, is it not likely that this is a power which the mind sometimes exercises in waking hours?"[1]

Kraepelin reports two remarkable cases of paranoiac patients, who were subject to pseudo-reminiscences of such an unusual nature that they deserve special mention.

The first is the case of a servant-girl, twenty years of age, subject to periods of excitement and depression, and the victim of hallucinations, the disease finally developing into erotomania. At times she had the illusion whenever anything occurred that her lover had foretold the event. "She related that she had met a beautiful wagon which he had foretold that he would send to her. Gravel had been emptied on a road and a bank formed. Even this her lover had predicted. Likewise he had told her that he should take her to a hospital, and further, that a new superintendent would come to the asylum. These predictions never occur to the patient *until she sees the things concerned, or until the events have occurred.* Then she suddenly remembers that some time before she heard her lover speak of them. Beforehand she knows nothing about them, but believes that she might really know." When it was announced to the patient that she was to be removed to another asylum, she at once thought that she remembered the very words with which her lover had predicted this event.[1]

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[1] Proceedings of the American Society of Psychical Research, Vol. I, No. 4, p. 567.

Kraepelin's other case is one of the most remarkable on record.

The patient was a merchant 33 years of age. For some 12 years he had shown symptoms of insanity. As the psychosis developed, he thought the *Fliegende Blätter* contained references to him, and that *Ueber Land und Meer* caricatured him. These illusions did not occur when he read the papers, but after some days the thought suddenly came to him that certain passages referred to him. He remembered even on what page a passage stood. Never finding the sentences remembered on turning to the papers, he got the idea that the editions in question had been withdrawn and that others had been substituted. After entering the asylum, the patient declared that some weeks before he had heard an account of all his companions, and that he had read in the newspaper about the management of the asylum, even in its minutest details. He had given no heed to these reports at the time. Not until he saw the people concerned and the places referred to, did it occur to him that he had already been told about them or that he had read of them. Then he reproduced in detail the circumstances and order of the former experience. In short, almost every striking new impression was the occasion of a pseudo-reminiscence. For example, when a murderer was arrested in Munich, he remembered at once that some time before he was asked whether he had ever been on the street where the arrest was made. The patient claimed that he had an unusually good memory (in fact the patient's memory for actual events was good), and his paramnesia extended to minute details. For example, he remembered that some months before he had read in the *Fliegende Blätter* a detailed account of the furniture of the dining-room at the asylum. After a time the patient's false memory took a peculiar turn. He found that views which he expressed in conversation in regard to the Pope, social reform, and the like, were printed almost verbatim in the newspapers. In the *Fliegende Blätter* he found jokes that he had formerly related and pictures that he had designed. In number 2022 there were eight of his own jokes; and he remembered that he had formerly said in answer to a question relating to the matter, that they would appear in a number that contained three two's. Finally it occurred to him that many of his thoughts had been previously communicated to him. Some verses even that he composed upon his fellow-patients were predicted to him. And he recollected that he had composed the obituary notice of the wife of a State minister a year before her death, on occasion of the illness of his own wife. The patient liked to spend his time thinking over the experiences of his life, and the abnormal activity of his mind is well shown in what he says of himself, "When I begin to compose, then one thing suggests another; it is the veriest Huns-battle of the mind."

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[1] Loc. cit., p. 397.

Dr. Orschansky has reported a case that certainly is closely related to this form of paramnesia[1]; although, for some reason that is not clear from the published report, he thinks it does not belong here.

The patient was a young Russian student who had been expelled from the university for engaging in a student riot, and soon after became the victim of delusions of persecution. The following are extracts from his stereotyped form of conversation with the physician:

"Then you are afraid of the treatment?—Yes. It was prophesied to me three years ago, that after three years Dr. O., dressed in a blue coat, just like the one I then wore, should treat me with electricity, but without success.—But what if I treat you hydropathically instead of with electricity?—That too, I think, was prophesied to me. . . . Everything that was to happen was prophesied; that at the railway station in Kursk I should see a waiter handing out tea to the passengers at the buffet, and that this should be no other than

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[1] Archiv f. Psych., Bd. XX, H. 2, S. 337 seq.

Alioschka, the Moscow executioner. On the way I saw a disguised woman. I believe it was my wife; this too had been prophesied to me. Here I am watched by the Princess T., who is dressed as a servant. The same person follows me everywhere. Sometimes she is blonde, again brunette, alternately young and old, sometimes a man, and again having the form of a woman. This also was prophesied to me.—Was anything else prophesied to you?—Oh yes! It was prophesied that the doctors in Charkow would declare me insane, that then a dispute would arise between them and the Moscow physicians, that the latter would declare me sane and win the day."

While too few cases have thus far been reported to admit of any very positive conclusions in regard to the cause of paramnesia and its relation to normal and abnormal mental activity, nevertheless one thing seems tolerably clear, namely, that the pathological cases of paramnesia are extreme forms of what occurs in normal life. Here, as is usually the case in psychiatry, the beginnings of pathological mental activity are found in normal life; and further study of paramnesia will probably furnish admirable illustration of the way that normal mental activity passes over into abnormal.

While the more remarkable forms of paramnesia cannot yet be satisfactorily explained, the relation of false memory to ordinary uncertainty of memory is, in most cases, easily traced. It may be seen from the most commonplace illustration. Try to recall all the events of yesterday in the order of their occurrence. The first and most serious difficulty you will encounter will doubtless be in localizing the details in their exact order in time. As I write, I try the experiment. Yesterday I went to Washington and studied at the Surgeon-General's Library. Plenty of things are recalled—even minute details. But did my friend say this before or after he said that? Did I write this note before or after I read the article in the *Lancet*? Did I examine the Japanese masks in the National Museum before I saw the Egyptian mummy? These and similar questions I cannot answer.

This, however, is not the only difficulty. I am not quite sure whether certain trivial things happened or not. For example, my friend told me what he ate for lunch. It *seems* to me that he said he had toast, but I am not sure. Now, if circumstances had happened to make it very important that I should testify upon this point—for example, suppose my friend had been poisoned—I might possibly by thinking intently, by calling to mind different kinds of food and by other devices, recall what my friend said. This recollection might come like a flash with an unmistakable clearness. It is more likely that I should gradually come to feel that the particular amount of familiarity that the mental image on trial came to have, and its congruousness with the mental picture of the scene when my friend told about the lunch, would finally appear to me sufficient evidence of its genuineness as a truly remembered event. It is clear that an error here is easy. That some persons would approve such a group of presentations upon less evidence than others is clear. That under stress of a necessity to support certain interests I might approve to-day what to-morrow I should deem sustained by too little evidence, is not difficult to believe.

The distinction between what is clearly remembered and what is imagined ordinarily is definite enough. There is, however, a doubtful borderland where it is difficult to tell what we remember. The acts of reproduction, recognition and localization in the past ordinarily occur so easily and rapidly that we fail to note what really happens; it may be that some of the processes are abridged or occur unconsciously. But when in doubt whether we remember or not, the intellectual process is essentially this: first, an act of reproducing a group of presentations; second, an act of judgment deciding whether the reproduced group has a sufficiently familiar aspect to make good its claim to recognition; third, localization in the past, if the judgment approves. Setting aside the errors in localization, it is plain that pseudo-reminiscences may arise from either of two causes—either the given group of presentations may occur to the mind with a factitious air of familiarity, or the critical function of the mind may be so impaired that the given group is recognized upon insufficient evidence. As has been shown above, a group of presentations may acquire a factitious aspect of familiarity from our having seen, imagined or dreamed something similar. And it is noteworthy that pseudo-reminiscences seem to occur most frequently among those whose judgments are weak—the young, the aged, and those whose minds have been weakened by disease.

Some of the cases recorded suggest that the phenomena of paramnesia, when carefully studied, may be helpful in the diagnosis and prognosis of disease. Kraepelin found simple paramnesia a very characteristic accompaniment of *dementia paralytica.*[1] A good part of the cases reported, both of simple and identifying paramnesia, have been epileptics. A symptom of such frequent occurrence (although it does occur in normal life) should be noted, as Hughlings Jackson has urged. How closely this phenomenon may be connected with the disease appears in one of the cases reported by Jensen. The patient came to him one day with the following complaint: "Doctor, I feel so very strange to-day. When I stand now like this and look at you, then it seems to me as if you had stood there once before, and as if everything had been just the same, and as if I knew what was coming; and when I think about it, I get so frightened [*schucherich*, a word used by the patient to designate the attacks], and I go back and turn around; and when it is over, the whole thing seems so ridiculous—and it has been so all the time to-day—I don't know what ails me."[1] On finishing these words the patient immediately had an attack.

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[1] See loc. cit., pp. 218, 219.

The medico-legal aspect of this subject is of the most practical importance. The more common forms of paramnesia described above show that it is not impossible to manufacture testimony. A member of the bar tells me that this is actually done in some cases, the method employed being somewhat as follows. The witness is a person of deficient memory. It is desirable that he should testify to the occurrence of a certain event. The lawyer asks the witness if he remembers this event. The reply is, No; and nothing more is said. But the idea of the event has been suggested to the mind of the witness. In a few weeks the lawyer repeats the same question, and again receives a negative answer. But after a few similar experiments the witness becomes uncertain whether he remembers the event in question or not. He begins to think that he does. The images of the imagination suggested by the lawyer's questions loom up vaguely in the mind, the memory is confused, and in a few months the lawyer, if skillful, may develop a pseudo-reminiscence so strong that the witness will give the desired testimony with complete sincerity. Of course this cannot succeed with persons of strong memory and critical judgment, but with children and aged people it may not be difficult.[1]

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[1] Allgem. Zeitschr. f. Psych., Bd. XXV, S. 59.

The false testimony of children has received some study. A. Motet[2] reports four cases from his own observation and cites others; among the latter is the famous case of Moritz Scharf, who falsely testified that his own father had committed a most horrible crime.[3] In Motet's opinion, children with abnormally developed imaginations often fail to distinguish what has actually happened from what has been imagined, read or suggested by others. Thus false testimony may be given in all innocence.

Nothing, as Motet says,[4] is more effective than a child's story of the details of a crime of which he pretends to have been a witness or a victim. The child's

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[1] The uncertainty of human testimony was notably illustrated a few years ago in the case of the Bell Telephone Co. vs. the People's Telephone Co. The chief point at issue was whether Daniel Drawbaugh had a telephone in his shop prior to 1876. Several hundred witnesses gave testimony bearing directly or indirectly upon this point. The honesty of most of the witnesses seems to have been admitted, yet evidence offered by one side was generally refuted by testimony from the other. The Supreme Court divided upon the case, and the seven thousand printed pages of evidence in the suit seem rather to prove the fallibility of human testimony than anything else. See article on Daniel Drawbaugh, by H. C. Merwin, Atlantic Monthly, Sept., 1888.

[2] Les faux témoignages des enfants devant la justice. Paris, 1887.

[3] See L'affaire de Tisza-Eszlar, Revue des Deux Mondes, August 1, 1883.

[4] Op. cit., pp. 7 and 8.

naiveté adds to the interest and elicits confidence. His hearers urge him on by their sympathy. Parents, friends, and neighbors accept the account, true or false. They suggest new details and fill up the gaps in the story. The child's uncritical mind assimilates these details, repeats the story without variations, and makes his accusation before the magistrate with an apparent accuracy that is most telling.

Finally, the study of paramnesia is of interest in relation to so-called cases of telepathy. As an explanation of some of these cases, Professor Royce has formed the following hypothesis :

"*In certain people, under certain exciting circumstances, there occur what I shall henceforth call* PSEUDO-PRESENTIMENTS, *i. e. more or less instantaneous and irresistible hallucinations of memory, which make it seem to one that something which now excites or astonishes him has been prefigured in a recent dream, or in the form of some other warning*, although this seeming is wholly unfounded, and although the supposed prophecy really succeeds its own fulfilment."[1]

Professor Royce argues that the pseudo-presentiments described above under the third form of paramnesia "occur sporadically among the sane, even as they occurred persistently in Kraepelin's young patient." He cites some dozen cases of dreams involving presentiments that in his opinion may be explained in this way. In these cases there is no satisfactory evidence to show that the dream was related to any one before the predicted event occurred ; and the events were prefigured with considerable minuteness of detail. This hypothesis cannot, of course, be easily verified in any given case. It gains *a priori* probability, however, from the phenomena of paramnesia as illustrated in this article. That it may be a very plausible explanation will appear from the following case (not unlike some of Professor Royce's) that has just come to my knowledge.[1] My informer is a well-known citizen of Baltimore. I consulted him personally, and report the case as nearly as I can in his own words. This is his story somewhat abridged :

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[1] Op. cit., p. 366.

"From boyhood I have had presentiments. They generally come when I first awaken in the morning. I hear an inner voice telling me what is to happen. Upon investigation I always find that the presentiments are corroborated in some way by facts.

"About four or five years ago a very remarkable event occurred. My place of business at that time joined my living apartment. I could look from my office through a glass door to a wooden door opening into the parlor some twenty-five feet away. While at work in my office one day I looked through the glass door toward the parlor opposite. The wooden door was closed, but *through* this door I distinctly saw a man passing through the parlor. I saw the whole parlor and everything about the man, his size, clothes, hat, beard—all distinctly. At once I started to go to him and see what he was doing. But, very singularly, as soon as I started I forgot what I was going for, the vision went out of my mind, and I went to my wife's room adjoining the parlor to look for her. Not seeing her, I was returning to the office when some power took me by the shoulders, turned me around, forced me back into my wife's room and up to a trunk in one corner. Here I found a man trying to open this trunk; and it at once flashed across my mind that this was the very man I had seen in the vision. The man feigned drunkenness, and I made him leave the house. When my wife came in soon after, I told her that I had looked for her and found a man instead. On her

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[1] Compare a dream reported by the Eng. Soc. for Psych. **Research**, Proceedings, Dec. 1888, pp. 316, 317.

> way to the house she had found a bit of cotton from her jewelry box, and she at once guessed that this man had stolen her jewels. This proved to be true, though I could hardly believe it at first, because, as I told my wife, there was not time for him to do so between my seeing him in the parlor and the time I found him at the trunk."
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> My informer is very positive about the vision. "No power under God's heaven," he says, "neither you nor anybody else, Doctor, can convince me that I did not see just these things. I saw the man, but I did not see him. I saw him in my mind. I can account for it only by thinking that it was the work of some higher power that wished to warn me of the robbery." "How," I asked, "do you account for the remarkable fact that after starting towards the man you immediately forgot the vision?" "I can account for it only in this way," he replied; "if I had gone into the room at once and found the man, I should have caught hold of him in a rage, and one of us would have been hurt; the higher power wished to protect me from this. Hence I forgot what I started for."

The length of time since this event occurred makes the record less trustworthy.[1] But granting that the account is substantially correct, an explanation on the theory of pseudo-presentiment is very plausible. That my informer's mind is fertile soil for unusual experiences is evident from the general tone of his account, that he was agitated at the sight of the burglar is a natural inference, that in the mental turmoil of the moment he might have a pseudo-reminiscence seems possible, but that he should see such a remarkable vision and at once forget it is almost unaccountable.

It has been the purpose of this paper to illustrate the different forms of paramnesia and to show the bearings of the subject rather than to offer theories. In conclusion I have only to express the hope that this article may stimulate others, especially alienists, to record cases of paramnesia that may come under their observation. Little is known about the more remarkable forms of paramnesia. There are as yet no data for determining how common pseudo-reminiscences are either among the insane or among normal individuals. But that the subject is interesting and practically important is clear from the cases already reported.

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[1] The wife confirms the telling of the story to her soon after the alleged events occurred.

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

Zur Geschichte des Gehirns, sowie central und peripherischen Nervenbahnen beim menschlichen Embryo. WILHELM HIS. XIV Band d. Abhandlungen d. Koenigl. Saechsischen Gesellschaft der Wissenchaften, No. VII, 1888.

Die Entwickelung der ersten Nervenbahnen beim menschlichen Embryo. Uebersichtliche Darstellung. WILHELM HIS. Arch. f. Anatomie u. Physiologie, 1887.

Under the second title His has given a condensed presentation of his view regarding the development of the nerve tracts in man. The first paper is more elaborate, and an idea of the work can be best gotten from considering certain portions of that. In the study of such questions, schematizing is the great pitfall that the author must avoid, according to His, and certainly he, as much as any one, has developed the technique by which such a danger may be escaped. The artistic figures which accompany the paper are actual reconstructions from the sections, accurate in every detail, for where the sections cease to be clear the figures stop.

It is the changes occurring between the end of the first and end of the second month which are here considered. The opening section deals with the changes in the form of the brain up to the end of the second month, the relative development of the different vesicles and the variations in flexure being shown by illustrations. At the end of this period the hemispheres have just covered the interbrain, and the cerebellum is commencing to grow out. A cross-section of the brain tube in the earlier stages is essentially like that of the spinal cord. It is laterally compressed, with thick side walls connected ventrally and dorsally by two thinner layers of cells. The dorsal wall is the dorsal plate (*Deckplatte*); the ventral, the ventral plate (*Bodenplatte*). The lateral walls on each side are divided through their entire length into two portions, ventral and dorsal, by a slight furrow visible on their mesial surface in cross-section. The more ventral portion on each side is the fundamental plate (*Grundplatte*), and the dorsal, the wing plate (*Flügelplatte*). It is necessary to introduce these terms, since the fate of the wing plate and fundamental plate are points of special interest. Without the figures, a description of the changes which this typical form undergoes in the different vesicles would be of no value. This type can be followed even into the forebrain, where the ventral plate becomes the infundibulum, the fundamental plates the corpora striata, and the wing plates take part in the formation of the hemispheres, while the dorsal plate helps to form the inner wall of the anterior portion of the hemispheres.

The histological changes follow the same order in the brain region that they do in the spinal cord. It is the region of the medulla, and specially the caudal portion of it, that leads in the development, a fact that can be brought into connection with the highly complicated character of this region and its fundamental importance to the organism. The relation of the cranial nerves is very interesting. The motor nerves of the spinal cord are found to arise from the cells in the fundamental plate. All the motor cranial nerves also arise from cells in the fundamental plate. In the lower cervical region, however, this solid nucleus of cells, which has thus far run the entire length of the cord, separates into two portions, a dorsal and ventral, the former giving rise to the accessory fibers which emerge along the line of juncture between the fundamental and wing plate. This double origin is carried up into the cranial nerves, and one set are found arising plainly from what is a continuation of the ventricular nucleus, while the other is equally plainly a continuation of the accessory. To the former belong the hypoglossal, abducens, trochlearis and oculo-motor, while to the latter, the dorsal, belong the motor portion of the vagus, glossopharyngeus, facialis, and the motor portion of the trigeminus, at least in part. The sensory roots also are similar to the spinal in their mode of origin. For the spinal cord the sensory roots arise from bipolar ganglion cells which form the spinal ganglia that lie outside of the nerve tube. As His points out, there is some reason to think that in the adult certain fibers in the posterior roots are unconnected with these ganglion cells, but he adds that he has no evidence at all on the point from his embryos, and is thus compelled to ignore the question for the present. The sensory cranial nerves arise also from outlying ganglia. These are four in number—the trigeminal, the acustico-facial, the glosso-pharyngeal, and the vagus masses. In development all these masses undergo differentiation which cannot here be followed, but they all agree in sending bundles of fibers towards the nerve tube, which then apply themselves to the wing plate, and turning, run longitudinally. There is this difference between the bundles thus formed, that those arising from the spinal nerves turn so as to run cephalad, while those formed by the cranial nerves run caudad and form the "ascending" roots of the anatomists, for their respective nerves. It will be further seen that there are no sensory nuclei in the nerve tube which are at all homologous with the motor nuclei, the connection of the sensory fibers being far less localized. It is also interesting to consider that since the bundles of sensory fibers from the spinal nerves form the rudiments of the posterior columns, so the roots of the sensory cranial nerves, being homologous with them, form what may be considered as the prolongation of these posterior columns. From all of which it follows that cranial and spinal nerves are fundamentally similar in their origin and development.

In next describing the manner in which the peripheral nerves grow out, His finds many illustrations of purely mechanical influences controlling their course, division, etc. Those who are at all familiar with the labors of this author will recognize that he has here a peculiarly favorable field in which to develop his view of the very great importance of mechanical causes in controlling the form of our body and its organs. In discussing this point he specially emphasizes the importance of the sequence of events. For example, an outgrowing nerve stem splits on pressing against a cartilage that lies in its path. Suppose the cartilage had not developed until after the nerve had grown quite past this point; it would then be easily conceivable that the form of the nerve stem might have been quite different. The sequence of events becomes, then, a matter of prime importance, and he summarizes it for this period as follows: 1. Formation of the myelospongium (the non-nervous framework); 2. development of axis cylinders from the nerve cells; 3. formation of first nerve trunks leaving the center; 4. development of the outlines of the skeleton; 5. gradual growth of the nerve trunks towards the periphery; 6. development of the protoplasmic processes from the cells in the central system.

For brain anatomy the paper is specially important; for many current views regarding the nature and significance of the cranial nerves will find in it their best evidence, as well as their most serious difficulties.

Ueber die Bestandtheile des vorderen Kleinhirnschenkels. W. BECTEREW. His and Braune's Archiv, 1888, No. 2 bis 4, S. 124.

On the basis of embryological studies Becterew describes, in the superior peduncle of the cerebellum, four bundles of fibers which acquire their medullary sheaths at different periods. A transverse section between the corpora quadrigemina and the cerebellum in the adult shows dorso-laterally on either side of the middle line the conspicuous crescent formed by the fibers of the superior peduncle. Referring to such a section, the author describes these four bundles as occupying the following positions: (1.) The first is earliest developed, and is found in the sharp ventral angle of the crescent. It does not arise from the cerebellum, but is lost in the principal nucleus of the vestibular nerve. Small in extent, it passes as far as the cephalic edge of the pons where the fibers cross the middle line as a commissure. This is the ventral bundle. (2.) The second in order is the dorsal bundle which forms the dorsal portion of the crescent, and arises from the nucleus fastigii and the cortex of the vermis on the corresponding side. (3.) Between these two, on the lateral curve of the crescent, appears the so-called middle bundle, the fibers of which mix partially with those of the bundles just described. In the cerebellum these fibers are in connection with the nuclei globosus and emboliformis. (4.) The last to develop is the one filling the remaining space along the mesial curve of the crescent, the inner bundle. It arises in part from the corpus dentatum and the cortex of the cerebellar hemispheres. The three bundles last named form the superior peduncle proper, and crossing the middle line end in the cells of the red nucleus. Becterew regards these three as a physiological continuation of the bundles which form the inferior peduncle of the cerebellum.

The Development of the Peripheral Nervous System of Vertebrates (Part I, Elasmobranchii and Aves). J. BEARD. Quart. Journ. Micr. Sci., Vol. XXIX, 1888, pp. 153-227, plates 16-21.

This elaborate paper is a continuation of the author's morphological studies upon the development of the peripheral nervous system of vertebrates, and is very largely discussional. According to Beard, the spinal ganglia of vertebrates are formed as differentiations of the inner layers of the epiblast just without the limits of the neural plate. The "Zwischenstrang" of His has no share in the formation of the ganglia, and the "Zwischenrinne" of the same anatomist has no existence. After separation from the epiblast, the neural cranial ganglia and the spinal ganglia are carried up with the closing in of the neural tube, and come to lie between its lips, but are quite distinct from the central nervous system. The neural cranial ganglia grow towards the lateral epiblast at the level of the notochord and fuse with it. In addition to the four elements of the anterior and posterior roots, two ganglionated and sensory, two motor and unganglionated, the cranial nerves contain a fifth element, derived from the lateral or branchial sense organs. Beard confirms the opinion of Balfour respecting the origin of the anterior roots of cranial and spinal nerves. F. T.

Sur la persistance de l'aptitude régénératrice des nerfs. C. Voulair. Bull. de l'Acad. d. Sc. de Belge [3], XVI, 7, p. 93.

The author cut the sciatic nerve in dogs and obtained the usual regeneration. Hoping to get this a second time in the same dog, he again cut the nerve, but without the desired result. The failure of the tissue to renew itself a second time in this case he attributes to the disturbance of the circulation, etc., following the operation, and to the resistance which the peripheral connective tissue offers to the proliferation. On the other hand, he did obtain regeneration for the second time in the poplitaeus internus of the dog, and concludes, from two successful experiments, that the same nerve may regenerate itself at least twice, perhaps more often.

Ueber die centrale Endigung des Nervus opticus bei den Vertebraten. J. Bellonci. Mit 8 Taf. u. 4 Holzsch. Zeitschr. f. Wissen. Zool., B. 47, H. 1, 1888.

During the past ten years Bellonci has published a number of papers on the finer anatomy of the central nervous system, and his work has been for the most part comparative. All this gives him facility in handling a complicated problem like the one indicated in his title. He has worked over the optic centers and nerves in the reptiles and batrachia, the teleost fish, the birds, and the mammals, thus obtaining four types for comparison. The method which was most successful was a staining and hardening in osmic acid, followed by clearing the section with ammonia. The effect of this treatment was to leave visible only the fibers stained with osmic, the remainder of the section becoming completely transparent. Most of the numerous figures accompanying the paper are made from such specimens, and certainly show the fibers with great clearness. The disadvantage of the method is that only small pieces of tissue can be used, and therefore the brains employed must always be of small size. The plan of the investigation is an analysis of the fiber systems found in the optic tract and the adjoining regions, and a following of each of these to its termination. In pursuing such a plan, much detailed description is required, which it is of course impossible to summarize. The optic fibers proper are traced to their destination by the study of serial sections in several planes. This method leads to the general conclusion that all optic fibers end in the corpora optica, the homologues in the vertebrate series of the corpora quadrigemina anteriora in man. The fibers coursing through the geniculate bodies and thalamus, according to Bellonci, give off at most fine branches to the cells of these parts, but do not lose their identity, whereas when the fibers reach the corpus opticum they branch, forming a profuse network, and there really terminate. The criterion of termination is then, for Bellonci, the formation of a fine network and the consequent loss of identity.

At first sight these results appear quite revolutionary, but it is not impossible to harmonize them with the current views based on other methods. The author, however, recognizes that his work bears on the problem from but a single stand-point, and that the true conclusion can be reached only after the matter has been tested from every side. The structure of the corpus opticum is fundamentally similar in all the types.

A nucleus of varying value and position, sometimes covered by the corpus opticum and sometimes exposed, forms his corpus posterius, which he clearly shows to be the homologue of the corpus quadrigeminum posterius in man. The commissura posterior of v. Gudden appears to end here after passing through the internal geniculate body, into connection with which some authors have previously brought it. The optic region in the birds was found to conform with the type shown in the teleosts rather than with that found in the reptiles and batrachia. In no case is any mention made of an uncrossed bundle of fibers in the optic tract such as exists in the higher mammals. This negative point is of interest, since the method used was well fitted to demonstrate the bundle if it was present in the small mammals—mouse, rat, guinea-pig, etc., which were studied. The manuscript and plates were completed in 1885, but for some unexplained reason have been delayed in publication.

Beiträge zur Kenntniss der Sehnervenkreuzung. J. SINGER und E. MÜNZER. Denkschr. d. Mathematisch-Naturwissensch. Classe d. Kais. Akad. d. Wissensch. zu Wien, Bd. LV, S. 163, 1888. Abstract by Steinach, Centralbl. f. Physiologie, No. 25, März, 1889.

Michel's paper (Festschrift zum 70 Geburtstage Kölliker's, Würzburg, 1887) has recently reopened the discussion on the partial decussation of the optic fibers. The authors have again taken up the problem, and by the aid of a new method, first described by Marchi and Algeri, have obtained some important results. The point of the method consists in treating small pieces of the tissue, in the early stages of Wallerian degeneration, first with Müller's fluid and then with a mixture of Müller's and osmic, with the result of tinging the normal nerve fibers light brown, whereas those undergoing degeneration appear intensely black. The course of the optic fibers was studied in the pigeon, owl, guinea-pig, white mouse, rabbit, dog and cat, the procedure being to enucleate one eye, kill the animal at the end of two or three weeks, and then study sections of the optic fibers through their entire course. It appears that the decussation is a total one in the pigeon, owl, white mouse and guinea-pig (a result quite in agreement with that of Bellonci noted above), while in the rabbit, dog and cat it is partial, the uncrossed bundle increasing in size in these animals in the order in which they are named. They further show that the uncrossed fibers do not normally form a bundle, but are scattered among the others. When, however, the nerve of one eye is caused to atrophy, the corresponding optic tract still contains the uncrossed fibers for the remaining eye. Owing now to the atrophy of the bulk of the fibers in this tract, the fibers remaining aggregate, and thus do form a bundle in such a specimen. It was this appearance which led v. Gudden and Ganser to describe these fibers as forming a bundle in the normal animal.

Ueber die Chiasma Nervorum opticorum des Menschen. ST. BERNHEIMER. Bericht über den VII intern. Opthalm. Congr. zu Heidelberg, 1888, S. 317. Abstract by Steinach, Centralbl. f. Physiologie, No. 25, März, 1889.

The same problem is attacked by Bernheimer by quite another method. He has studied the development of the medullary sheath of the optic fibers in foetuses and young infants. Before the 30th week of embryonic life, the medullary sheaths are undeveloped, and the chiasma at this period is formed by a network of axis cylinders imbedded in vascular connective tissue. Not until the 30th week do branched particles, staining by Weigert's method, appear. In older embryos these gradually become larger and more branched and finally fuse, thus marking out very fine medullated fibers. These latter increase both in size and number during the remainder of intra-uterine life, but it is not until the second or third week of extra-uterine life that a section is made up of fibers all of which are medullated; these in turn increase in diameter up to the end of the first year of infancy. In this last stage the fibers are too numerous and interwoven to permit the observation of the course of single fibers. If, however, complete series of thin sections are examined from specimens between the 30th week of intra-uterine and the 3d week of extra-uterine life, there are always to be found fibers which enter the right optic tract from the right nerve and the left from the left, *i. e.* do not decussate. They run mainly in the dorsal half of the chiasma.

Aneurism of an Anomalous Artery causing Antero-Posterior Division of the Chiasm of the Optic Nerves, and producing Bitemporal Hemianopsia. S. WEIR MITCHELL. Journ. of Nervous and Mental Diseases, Jan., 1889.

In 1886 a man of 43 years presented himself as a patient, exhibiting as chief symptoms fatigue after unusual exertion, a tendency to numbness in the limbs, and, for a year previous, varying and gradually increasing pain in the parietal and vertex regions. An examination of the eyes showed complete anaesthesia of the nasal half of each retina. The color sense was unimpaired. The diagnosis was pressure in front of the chiasma, sufficient to cut off the connection between the inner halves of each retina, with partial atrophy, especially in the left nerve, inferred from the diminished acuteness of vision. The headaches continued, and later there was at times a passing sense of mental confusion. In May, 1887, he died suddenly, having been for some hours comatose.

At the autopsy the optic nerves were found separated fully an inch by a large tumor that lay directly between them. The tumor had apparently destroyed the chiasma and pressed deeply into the brain substance in the middle line, though not adherent to it. The two internal carotids were found intimately connected with and apparently forming the tumor. To explain this it is assumed that an anomalous artery connected the carotids at the point just below the chiasma, and this becoming aneurismal, caused the destruction of the chiasma, and forced its way into the brain substance above. The case is unique, and shows the partial crossing of the optic fibers in man, as well as the portions of the retina with which the two groups of fibers are respectively connected. Neither the optic nerves nor the brain were in a condition to permit of a microscopical examination. The paper closes with a discussion of the anomalous connecting branches observed in the circle of Willis, with a view to fully justifying the statement that the tumor was due to an aneurism in an anomalous artery.

Zur Physiologie des Vogelgehirns. M. E. G. SCHRADER. Pflüger's Archiv, XLIV, S. 175. Reviewed by Paneth in Centralbl. f. Physiol., März, 1889, No. 25.

This interesting paper opens with a discussion of previous investigations of the bird's brain, and has to urge against much that has been done, both the failure of the experimenters to treat the problems in a thoroughly objective way, and to control their results by suitable autopsies. It has been possible to remove the forebrain completely without injury to the remaining portions. The mortality immediately after operation is high, and of those that live for weeks after it, a portion show progressive emaciation and stupor, terminating in death. In the first days after operation the animals exhibit as a rule stupor and lack of voluntary motion, as described by Flourens. After this they move about the room, avoiding obstacles, even such as a dusty plate of glass, and can climb out of a cage with high sides. In the early stages they can easily be put to sleep by holding them or putting them on a perch, which shows that the movements are not forced. At night they roosted naturally. When placed on a rotating object they balanced very well for a time, and then flew off, evidently choosing the object towards which they flew. Alighting was accomplished with ease, but they could not rise from a flat surface like the floor. In some cases the period of stupor did not occur, and the animal moved about almost immediately after the operation in a way which showed that it could see fairly well from the very first. The reaction to sound was obtained with a percussion cap, by the explosion of which they were startled. The tests of other senses were not satisfactory, for Schrader did not get any good reactions on normal birds with which they could be compared. Birds without the forebrain did not eat voluntarily, but, as is well known, could swallow food fed to them. The operated birds slept, and became restless when left for too long a time without food. They showed heat, though they did not recognize the female, and a mother could not feed her young. The whole activity of such birds is reduced to the simplest terms.

Other birds were rendered blind. When the behavior of these was studied, it was found that they were in certain ways more defective than those without the forebrain. They could feed, but did so only at long intervals. They could not orient themselves, and remained for days resting on a perch, though it was close to the floor. The loss of sight evidently deprived them, as might be expected, of the majority of their most important sensations. So far as observed, no compensation through other senses took place.

Experimentelle Untersuchungen über die Beziehung der motorischen Ganglienzellen der Medulla spinalis zu peripheren Nerven. A. v. SASS. Inaug. Dis., Dorpat, 1888. Reviewed by Openchowski in Centralbl. f. Physiol. No. 25, 1889.

By a systematic application of v. Gudden's method, by which the atrophy of the central end of a motor nerve and its corresponding nucleus is brought about when the peripheral portion has been removed in a very young animal, Sass sought to determine the position and extent of some of the motor-nuclei in the spinal cord. The animals having been allowed to live some time after operation, serial sections of the cord were made and the number of ganglion cells in the corresponding halves compared, it having been previously determined that in a normal animal they were approximately equal in number on the two sides. By the decrease in the number of ganglion cells in one half of the cord, the extent and outlines of the nuclei could be traced.

The author concludes that the nuclei of the spinal nerves are not distinctly circumscribed, but that the nuclei run into one another. The motor region for the median nerve is in the caudal two-thirds of the eighth cervical segment, the cephalic third of the seventh and caudal third of the sixth; that for the radial nerve in the cephalic third of the eighth, in the seventh and in the cephalic half of the fifth cervical segment; that for the ulnar in the cephalic half of the first dorsal, and in the cephalic third of the eighth cervical. The nucleus of the ischiadic nerve was found, in conformity with the results of v. Gudden and Mayser, in the caudal portion of the lumbar enlargement.

Anatomische aanteekeningen naar aanleiding van een geval van atrophie van het linker corpus mamillare. C. WINKLER und J. TIMMER. Feestbundel van Donders, p. 26. Reviewed by Heymans, Centralbl. f. Physiol. No. 25, 1889.

From the study of rabbits and dogs v. Gudden reached the conclusion that each corpus mamillare contained at least two nests of cells — a medial and lateral. The medial nest connected mainly with the ascending pillars of the fornix, while the lateral one was connected with the bundle of Vicq d'Azyr, which in turn is connected with the tuberculum anterius of the optic thalamus. v. Gudden further described a crossed bundle, which now bears his name, the bundle of v. Gudden, and which passes from the corpus mamillare caudad and disappears among the tegmental fibers. The applicability of these results to man has been questioned by Flechsig, though v. Monakow has already described three pathological human brains which in general support v. Gudden's results. The fourth case is now described by the authors. It was the brain of an idiotic and epileptic girl, showing marked degeneration in the left hemisphere, and that specially in the parietal and occipital lobes. The basis of the brain was symmetrical save that the left corpus mamillare was atrophied to almost half the normal size. The corpus striatum was alike on both sides, but the tuberculum anterius was atrophied completely, while the pulvinar was somewhat smaller on the left side. Microscopical examination revealed a slight atrophy of the cortex of the gyrus hyppocampus, with complete atrophy of the cortex of the parietal and occipital lobes, an interesting partial atrophy of the left ascending pillar of the fornix, the lateral ganglion of the corpus mamillare and the bundle of Vicq d'Azyr, but there was no indication that this bundle crossed as described by v. Monakow. These facts, besides showing the multiple origin of the fornix (v. Gudden), are taken to indicate that the most inferior of the fornix bundles arises from the lateral ganglion of the corpus mamillare (against v. Gudden), and that a connection exists between the tuberculum anterius and the parietal and occipital lobes.

Ueber die experimentelle Verstopfung der Sinus Durae Matris. P. FERRARI. Wiener med. Jahrb. 1888, p. 81.

Contributions showing the conditions necessary for the circulation within the cranium are a natural result of the present interest in the surgical interference with the brain. By the injection of sterilized masses of wax and oil into the various cranial sinuses of the dog, the author has produced a mechanical obstruction in different portions of the venous outflow from the brain. The injection was made through the vena facialis or the vena ophthalmica, as the occasion demanded. By this means the sinuses of the roof or the base of the skull or the sinus cavernosus could each of them be separately injected, and when the injections were made at intervals, wax of different colors was used, thus enabling the experimenter to trace the result of each injection at the post-mortem. The general result was that very extensive plugging of the sinuses could be made without any serious symptoms or without giving rise to degenerative changes in the brain substance in the neighborhood of the plug. Indeed, all the sinuses of the roof could thus be plugged without causing any symptoms, but when all outflow was cut off the dog died in a few minutes, death being usually preceded by an epileptic attack.

Ueber drei Fälle von progressiver Paralyse mit Herderkrankungen in der inneren Kapsel. TH. ZACHER. Archiv f. Psychiatrie, XIX, 3, S. 726.

The descriptive anatomy of the brain suffers much confusion from the fact that it is, as a rule, very difficult to designate the exact level of the section by anything except a figure or an elaborate description. The latter is wearisome, but the lack of it may often cause misunderstanding. The position of the various tracts in the internal capsule is a case in point, for here the relative position is largely influenced by the level at which the section is made. This appears to be one source of the somewhat conflicting accounts of several authorities. From a study of his three cases, the author takes sides in the following way. A horizontal section of the brain made at such a level as to give nearly the maximum distance between the head and tail of the caudate nucleus displays the bands of white matter forming the internal capsule, the anterior limb lying between the head of the caudate and the lenticular nucleus, and the posterior limb between the lenticular nucleus and the optic thalamus. The two limbs meet at almost a right angle in this section, and form the portion known as the knee of the internal capsule. In the first case of the author, the lesion involved the major portion of the anterior limb, except the neighboring part of the knee, and no secondary degeneration was found in the corresponding crus. In the second case, the portion of the knee intact in the first instance was affected, and there was marked degeneration in the mesial portion of the pes. From this it is concluded that those fibers coming from the frontal regions and traversing the entire extent of the internal capsule lie in the anterior third of the knee. In the second case also, the degeneration involved the first third of the posterior limb and there was no degeneration in the pyramidal tract, while in the third case the lesion was in the middle of the posterior limb and the pyramidal tract had degenerated. The last two cases favor Flechsig's view that the pyramidal fibers are never located in the anterior third of the posterior limb of the internal capsule, as against Charcot, who teaches that they extend into this region. In the third case, the lesion, which was on the left side and it will be recollected was accompanied by pyramidal degeneration, had but a *transient* paralysis on the right side as its consequence. This very remarkable fact the author seeks to explain by reference to the results of Goltz on dogs, and in his explanation speaks of fibers for both halves of the body arising from each hemisphere, but both acting only under exceptional conditions.

(Recent work gives ground for expressing this idea in a somewhat different way. The spinal centers in the dog and other lower forms retain the power to act bilaterally, whether the impulse comes from one hemisphere or the other. In man and the apes, this bilateral character is as a rule lost, each half of the spinal center responding to its own hemisphere alone. A case like this would then be an example of the retention of a primitive condition by one of the higher forms.—D.)

Contributo alla fisio-patologia del cerveletto. A. BORGHERINI. (Revista sperimentale di Freniatria e di Medicina legale, XIV, 81.) Abstracted by Paneth, Centralbl. f. Physiologie, Feb. 1889, No. 22.

The author has spooned out the cerebellum, either in part or completely, from dogs, and afterwards observed them, sometimes for months. In both total and partial removal the symptoms were similar, but more severe and persistent in the former case, as might be expected. Following total removal there was at first great difficulty, which might amount to inability to move, that depended on a spastic condition of the muscles. With recovery from this extreme condition, the motions of the animal still remained slow, uncertain and simple. Sensibility was as a rule unaffected. In cases of partial removal, uncertainty and trembling were the prominent symptoms, which often completely disappeared in a week or two. The muscles of the eyes and of mastication did not appear ever to be affected. To meet the objection that the persistent ataxia in the two cases of total removal was dependent on the secondary degenerations that were found, the author reports the case of a dog becoming spontaneously ill, exhibiting symptoms similar to those in the above two cases, and showing at the autopsy a degeneration of only the gray matter of the cerebellum.

Ein Fall von hochgradiger Zerstörung des Kleinhirnwurms, nebst casuistischen Beiträgen zur Lehre von der sogenannten cerebellaren Ataxie. E. BECKER. Virchow's Archiv, CXIV, H. 1, S. 173.

In this case there was during life no disturbance either of motion, sensation or intelligence. The autopsy revealed two old apoplectic cysts, one in the left cerebellar hemisphere, and the other involving all the vermis except the lingula, lobus centralis, uvula and nodulus.

Nothnagel has indicated cerebellar ataxia as an almost unfailing symptom of extensive disease of the vermis, but Becker succeeded in finding in the literature some seven cases similar to his own. He concludes, therefore, that cerebellar ataxia is associated with the lesion of some special tract in this region, and points out that Edinger has described the cerebellar-olivary tract, from the superior olive, crossed corpus restiforme, capsular fibers (Vliess), superior peduncle, to the red nucleus, as probably specially connected with the equilibrium function. This tract Becker specially studied in his case and found it free from secondary degeneration, and therefore concludes that the case favors Edinger's hypothesis.

Ueber den Klangstab, nebst Bemerkungen über den Acusticusursprung. JUL. NUSSBAUM. Medicin. Jahrbücher, 1888, S. 187.

On examining the striae medullares, which are considered to form a central tract for the accessory auditory nucleus, it is found that only the most cephalically placed bundles decussate immediately beneath the floor of the ventricle. The same is true for the major portion of fibers which appears to have the same origin, but which, after crossing, takes a direction more cephalic along the floor of the ventricle and disappears at its antero-lateral edge, often in the region of the locus coeruleus. This bundle is frequently present, though inconstant and variable, and forms the conductor sonorus (*Klangstab*) of Bergmann. In the conductor there is a central core of cells completely surrounded by fibers. For this structure no function has as yet been assigned. Nussbaum further describes a bundle which follows the striae medullares in its later course, but within the medulla is at first associated with the ascending root of the acusticus.

(When a structure like the "conductor" is described as inconstant, the term must be taken as a rule to apply only to its macroscopic appearance, for the same structure sunk somewhat below the floor of the ventricle would not be discoverable on superficial examination.—D.)

Herderkrankung des unteren Scheitelläppchens. C. WERNICKE. Archiv f. Psychiatrie, XX, 1, S. 243.

A man of 70 years suffered a slight cerebral attack without external injury. The head tended to the right, and there was slight sensory and motor paralysis in the left side of the body, the facialis included. The most striking symptom was, however, the conjugate deviation of the eyes to the right, with inability to turn them to the left. This disappeared in a few days. A second attack was followed by almost complete paralysis of the left arm and leg—transient but considerable disturbance of speech, the paralysis of the left facialis remaining insignificant; three days later divergent strabismus of the right eye. A few days after the second attack the patient died. Wernicke had diagnosed a lesion in the inferior portion of the parietal lobe, and a second in the region of the internal capsule. This second lesion was necessary to explain the complete hemiplegia consequent on the second attack. His localization of the first lesion was based on a case of Grasset's (Montpellier Med., June, 1879), and the experiments of Ferrier and Munk, in which conjugate deviation of the eyes was found associated with the angular gyrus. A softening in this locality would also account for the other sensory disturbances following the first attack, since just beneath this region the sensory fibers for the entire body are most compactly grouped. The autopsy fully confirmed the diagnosis. There were several small cortical lesions and an old lesion in the pons—all without significance for the present discussion. Besides these, however, there was a fresh lesion in the third member of the right lenticular nucleus, which would easily involve the pyramidal tract, while the white matter of the lower portion of the right parietal lobe was completely softened, thus fully accounting for the other symptoms. A review of the literature brought to light a considerable amount of evidence showing conjugate deviation of the eyes in similar lesions of the parietal lobe.

Die Beziehungen der hinteren Rindengebiete zum epileptischen Anfall. H. Unverricht. Deutsch. Archiv f. klin. Med. XLIV, 1, S. 1. Reviewed by Ziehen, Centralbl. f. Physiol. No. 25, 1889.

The starting point for an epileptic attack is usually thought of as in the motor region of the cortex, but Unverricht considers it as clinically established that a strictly local affection of the posterior cortical regions can of itself bring about convulsions. He seeks to demonstrate the point on dogs, in which the stimulation, especially of the posterior and superior portions of the second arched convolution (counting from the middle line) causes contractions. There is lateral motion of the eyes to the opposite side, with dilation of the pupils as one of the results. These are not explained as reflexes from sensory stimuli (Ferrier), but as the result of direct stimulation of motor centers. But this is simply the author's view, for which the evidence is lacking thus far.

The length of time the stimulus is continued is more important than the strength of the stimulus, in determining an attack from this posterior cortical area. The order of contractions often fails to follow the order of the centers, and at times the convulsions roused from one visual area are limited to one half of the body. Most important are two experiments in which on the left side all the motor region save that for the movements of the eyes had been removed. The visual area was then stimulated on the left side, and the convulsion appeared on the *same* side. When a transverse cut was made through the cortex of one hemisphere at the anterior edge of the visual area, then stimulation of the latter caused a convulsion in which the orbicularis contracted after the extremities, from which he concludes that the impulse travels through deep-lying connections. At the end some clinical evidence is presented.

Kleine Beiträge, betreffend die Anordnung der Geschmacksknospen bei den Säugethieren. J. Hönigschmied. Zeitschr. f. wiss. Zool., Vol. XLVII, 1888, S. 190–200.

This paper contains the results of the author's further studies upon the arrangement of the gustatory papillae and distribution of the taste-bulbs in mammals. In *Felis lynx* there are present six papillae of the circumvallate type, but the foliate papillae are wanting. The taste-bulbs are rather narrow and disposed in a zone of 3 to 5 tiers. *Ursus fuscus* has about twenty circumvallate papillae, and also well-developed foliate organs. The taste-bulbs are oval or cylindrical in form and, in the circumvallate papillae, are arranged

in a zone of 21 to 25 tiers. In vertical section the folds of the foliate organs show 5 to 20 rows of bulbs. *Equus asinus*, like the horse and mule, has two circumvallate papillae and foliate organs. The taste-bulbs are long and narrow, and those of the circumvallate papillae are arranged in a girdle of 5 to 10 tiers. The folds of the foliate areas contain 5 to 10 rows of bulbs. In *Cricetus frumentarius* the foliate type of taste organ is present, but circumvallate areas are lacking, although Mayer mentions finding a single circumvallate papilla in the hamster. The folds of the papillae foliatae bear from 2 to 10 rows of nearly spherical bulbs. *Sus scrofa*, like the domesticated pigs, possesses two circumvallate papillae and foliate structures. In the former region the bulbs are long and narrow, and the tiers vary in number from 7 to 25. The folds of the foliate papilla show 5 to 16 rows of bulbs. In *Mus sylvaticus* there is but one circumvallate papilla, while *Myoxus avellanarius* possesses three papillae of this type. In neither of these species, apparently, was a thorough search made for foliate taste areas. In none of the animals investigated were taste-bulbs detected in the epithelium of the upper surface of the circumvallate or foliate papillae, nor were any found in that lining the outer wall of the trenches encircling the former. Hönigschmied found fungiform papillae bearing taste-bulbs in *E. asinus*, *S. scrofa*, *C. frumentarius* and *M. avellanarius*. Both Hönigschmied and Brücher regard the circumvallate papillae as modifications of the fungiform type. While this hypothesis may be true in the case of certain individual papillae, it seems highly probable that the circumvallate papilla, as a distinct taste area, has been developed through a long series from the gustatory or bulb-bearing ridges. These ridges, which have been found in *Ornithorhynchus anatinus* and *Belideus ariel*, are probably the nearest approach among living animals to the primitive type of gustatory area of Mammalia, and are doubtless the forerunners of the circumvallate type of papilla.
F. T.

II.—EXPERIMENTAL.

Ueber die Helligkeitsempfindung im indirecten Sehen. A. KIRSCHMANN. Philosophische Studien, V, 3, pp. 447–497.

Kirschmann, without having seen the work of A. E. Fick, examined the lateral portion of the retina in regard to its sensitiveness to brightness. His results are of a different nature from Fick's, for his eyes were examined under ordinary conditions, and Fick's only after prolonged adaptation. He finds an increase of sensitiveness amounting to a fraction of the whole—one-seventh for 20° away from the center—while Fick found a sensitiveness fifteen times as great as at the center. The latter result is plainly very different from anything which takes place under ordinary conditions, or we should not fail to be easily aware of it.

The different parts of the retina differ in respect to (1) the distinctness, (2) the quality, and (3) the intensity of the sensations which they convey. (1) On account of the unfavorable conditions for refraction and accommodation, the images on the side parts of the retina are not good; but this alone is not enough to account for the degree of indistinctness of vision that prevails there—the eyes of rabbits, for instance, are such that very sharp images are formed in the periphery. It is necessary to assume therefore that the diminished number of nerve-ends is an additional cause. (2) There is an important modification of the color-sense towards the periphery; at a certain distance blue and yellow only are seen, and farther away no color at all is distinguished. But the changes which colors undergo as they are looked at peripherally are very different from those which are caused by a diminished brightness; red becomes orange when it is looked at with the periphery, but when looked at in a faint light, black; neutral violet becomes blue and gray respectively. Hence a diminished brightness cannot be the cause of the change. (3) That a diminished brightness does not occur on the border is made plain by these experiments. Two black and white rotating disks were looked at, one with the fovea and one with some other portion of the retina, and black was added to the latter until it looked no brighter than the former. A maximum sensibility was found at a distance of 20°—25° in a horizontal direction, and 12°—15° in a vertical direction. The increase of sensibility is much greater horizontally than vertically. An interesting fact is that the lower portion of the retina is less sensitive to brightness than the upper portion. We have no occasion to notice slight changes of brightness in the sky, but it is essential to safe moving about that unusual things on the ground should attract our attention.

Kirschmann draws attention to the fact that the cone of light which can enter the pupil from an object far to one side is very thin, and hence that the objective brightness of things seen laterally is diminished in the ratio of the cosine of the angle, if the object is far away. Hence the actual increase in sensibility in the lateral portion of the retina must be greater than the apparent increase by an amount enough to make up for this objective decrease of brightness. After taking account of this, the curve of increase is found to be a straight line. The cause of this greater feeling for brightness in the non-foveal portion of the retina he finds in the supposition that the end-members of the rods, which are much more developed than those of the cones, act as reflecting mirrors to send some light back upon the nerve-terminations. It remains to be found out whether the rods are most numerous at the place where he finds the maximum sensibility.

An interesting fact which appears from his tables, but which Kirschmann himself does not mention, is that for bright objects (the background was constant, 270 black + 90 white) the nasal portion of the retina is decidedly better than the temporal. This is a difference which, as Schön has shown, is of great importance in enabling us to determine whether a given pair of double images belong to an object without or within the horopter—so great that by an artificial alteration in the relative brightness of the images an error of judgment in this respect may be produced. C. L. F.

Ueber den Lichtsinn der Netzhautperipherie. TREITEL. Arch. f. Ophth. XXXV, 1, p. 50.

This is the third independent investigation into the sensibility of the lateral portions of the retina that has been made within a very short time, and the difficulty of the subject is apparent from the fact that all three investigators, A. E. Fick, Kirschmann and Treitel, have obtained very different results. Treitel finds the lateral portion less sensitive than the fovea, Kirschmann finds it more sensitive by about one seventh, and Fick finds it ten to fifteen times more sensitive. The experiments of Fick seem to us to have been conducted with much the most acumen and with the most careful avoidance of sources of error. In the first place, his object was seen by transmitted light, instead of by reflected light. A small piece of porcelain was lighted up from behind, and the amount of light allowed to fall upon it was regulated by a diaphragm. Both the other observers used Masson disks, and measured the amount of white necessary to add to the black to make the disk just perceptible; their source of light was the windows of the room, or the windows somewhat darkened by curtains. But every one knows that it is almost impossible to hold two strips of paper, near together even, in front of several windows, in such a way that they are equally lighted. Kirschmann, it is true, had windows looking upon a gray wall, and allowed for the contrast effect of his background, but neither of these precautions was taken by Treitel. The results of Fick and Kirschmann are not really incongruous, for Kirschmann worked by day-light and Fick in the dark; and even Treitel found that the supposed superiority of the fovea was almost evanescent in a diminished light.

In the second place, it was discovered by Schadow that the presence of a bright image on the fovea renders the lateral parts of the retina much less sensitive than they would be without it. Treitel maintains that it is impossible to steadily fixate a point when there is nothing to look at; that may be so in general, but if there is a bright point above and one below, it is not difficult to keep the eye steady half way between them, and this is the device which Fick made use of. Treitel finds the retina one tenth as sensitive at a distance of thirty to forty degrees from the center in daylight, but half as sensitive in a faint light. The difference he attributes to the more ready fatigue, and hence the slower adaptation of the center. But it has never been shown that the center is more easily fatigued than the periphery. This is really a case of question-begging. It can only be more readily fatigued by being more sensitive, and whether it is more sensitive or not is the very question at issue. A supposed fact is not rendered more probable because it can be explained by something else which is a mere re-statement of the fact itself. Moreover, a common experience shows that the center is less subject to fatigue than the periphery; any one whose eyes have been pained during sickness by the presence of a light in the room knows that it can be much more easily endured by looking directly at it.

On the whole, the subject seems to be left at present in a good deal of confusion. The former observers who agree with Treitel are Rupp, Exner, Dobrowolsky and Gaine, Chodin, and Bull; those who disagree with him are Aubert (but he thinks the adapted eye is about the same throughout), Schadow, Charpentier, and Butz. But the experiments of Fick, as we have already said, seem to us to carry more conviction with them than any of the others.

C. L. F.

Ueber intermittirende Netzhautreizung. BELLARMINOW. Arch. f. Ophth. XXXV, 1, p. 25.

Kirschmann has just called attention to the fact that two sensations furnished by a rotating disk are fused into one sensation with less velocity of rotation at the fovea than in the periphery. Bellarminow has made an extended series of experiments on this subject. Exner had already noticed (*Pflüger's Archiv*, Vol. XXXII) that the periphery is more sensitive to change of brightness than the center, and that it has a special tendency to interpret change of brightness as due to motion. Hence it detects small motions more readily. Bellarminow regulated the intensity of the light by the width of a slit, produced intermittance by a rotating disk, determined the velocity of its rotation by the pitch of its musical note, and used the spectrum for experiments in color. In a faint light the rapidity of rotation necessary to cause flickering to cease was only two-thirds as great at the center as in the periphery. In strong light the difference was not so great. Blue and violet gave the same results as white light, but green, and still more yellow, fused at high intensities soonest in the periphery. The size and shape of the object looked at seemed to have no effect on the result. The author attributes these results to the greater intensity but shorter duration of after-images in the periphery. He does not make it plain how that, if it were true, would be an explanation. Nor does he give anything to show that the greater sensitiveness of the periphery is not a quality of cortical instead of retinal vision. It might well be that we had the habit of giving such instantaneous and undivided attention to indications of motion in the lateral field of vision as would be quite sufficient to account for a greater power of detecting it. There is at present, of course, no sure way of distinguishing between the retinal and the cortical divisions of the visual process.

C. L. F.

Physiologische Studien über die Orientirung, unter Zugrundelegung von Yves Delage Etudes expérimentales sur les Illusions statiques et dynamiques de Direction pour servir à déterminer les Fonctions des Canaux demicirculaires de l'Oreille interne. HERMANN AUBERT. Tübingen, 1888, pp. 122.

The fact that nearly all the other works on the subject of the functions of the semi-circular canals were either written in German or have already been translated into German (the brief papers of Laborde and Crum Brown form the only exception) has induced Aubert to translate this work of Delage's, which appeared originally in the *Archives de Zoologie expérimentale*, 1886 (see review in this JOURNAL, Vol. I, p. 179). He takes occasion to add many foot-notes of his own, chiefly confirmatory of Delage, in view of the fact that his own complete investigations on the subject will not be ready for publication for some years to come.

Since Delage's articles were written, the question whether the semi-circular canals are or are not organs for the perception of rotation has been set at rest by the admirable work of Breuer in producing determinate rotations of the head in doves by electrical stimulation. (See Pflüger's Archiv, Bd. 34, p. 135, reviewed in this JOURNAL, Vol. II, p. 332.) But in what way the sensation is excited still remains an unsettled question. Breuer produces the compensatory motions of the head (and hence, by inference, the sensations) by drawing out the endolymph with a piece of blotting-paper, but this does not prove that an actual motion of the endolymph must take place every time that the sensation is produced. A retarded flow of liquid in large glass tubes made in the shape of the canals takes place in a way that would exactly explain all the phenomena, but most writers agree (and Mach has shown by experiment) that that retardation would not take place in tubes so small as the actual canals, and hence there is nothing but changes of pressure to fall back upon. Aubert does not obtain the same experimental results as Delage when it is a question of throwing doubt upon the sufficiency of this explanation, and Mach maintains that the change of pressure which would take place is not too inconsiderable to produce the required effect (*Bewegungsempfindungen*, Leipzig, 1875). The work done in hearing the lowest perceptible sounds, according to Töpler and Boltzman, is 1 : 3,000,000,000 of a kilogrammeter per second; in seeing the faintest lights, it is, according to Thomson, 1 : 5,740,000,000; and Mach's calculations show that much more than that would be done in the semi-circular canals. C. L. F.

Zur Physiologie der Bogengänge. J. RICH. EWALD. Pflüger's Archiv, Bd. XLIV, H. 7-8-9.

On opening the bony semi-circular canals of a dove, variations in the level of the perilymph are sometimes to be observed, which have been connected by some observers with the rhythm of the heart. It seemed to Ewald at first that they rather followed respiration, but he later discovered that they were produced by the movements of the lower mandible, which sometimes accompany respiration, and occasionally for a few seconds are as rapid as the pulse. The mandible moves the adjacent parts of the skin, and they the ear-drum, which communicates the motion indirectly to the perilymph. In uninjured canals the changes in the level must be changes of pressure, affecting both perilymph and endolymph, but do not cause sensations of rotation, because the pressure is equal in all the ampullae. The impulses are too few and too gentle to cause sensations of tone or noise, but probably do cause momentary deafness, as yawning does in man.

Bydrage tot de physiologie van den reuk. H. ZWAARDEMAKER. Feestbundel van Donders, 1888, p. 179. Abstract by Heymans, Centralblatt f. Physiol. No. 26, 1889.

Using yellow wax to produce an olfactory stimulus, the author found that 0.1 second elapsed before the odor from a surface of 122 sq. mm. held before the nose was perceived. With smaller surfaces the time was longer (though not shorter with larger ones), and warming the wax quickened its perception. He found also that different odors diffuse themselves in still air at different rates. At a distance of 40 cm. and a temperature of 15° acetic ether was perceived in 4 seconds, sulphuric ether in 9, soap and tallow in 10, paraffine in 18, camphor in 19, yellow wax in 20, turpentine in 22, vulcanized rubber in 45, thus forming a series according to rate of diffusion from the most to the least volatile, the rate depending on the physical peculiarities of the molecules. The author has devised an instrument with which to test the sense of smell, depending on the relation of the area of the exposed surface of the odorous substance to the intensity of the odor. Individuals differ as to acuteness, but for the same person and the same substance the acuteness is constant. The author considers the olfactory stimulus to be a chemico-mechanical one.

The Limits of Hearing. J. KERR LOVE. Jour. of Anat. and Physiol., Vol. XXIII, 1889, pp. 336–339.

Love concludes, from experiments made with large tuning-forks and with a series of vibrating metallic tongues, that notes produced by fifteen or sixteen vibrations per second are the lowest which can be heard by the human ear. He describes minutely an instrument of his own device for determining the smallest appreciable difference in pitch. For untrained and slightly trained ears the least observable difference of pitch is from $\frac{1}{4}$ to $\frac{1}{16}$ semitone. A twenty-fourth semitone is a common limit. The ears of such trained musicians as violinists, tuners, and some pianists, can detect with certainty a difference of $\frac{1}{24}$ to $\frac{1}{80}$ semitone. All observers, but especially the untrained, detect sharpened better than flattened intervals. Generally speaking, Weber's law holds good for all but the highest and lowest parts of the musical scale. Love considers Politzer's acoumeter the best test we have for determining the distance at which a note of given intensity can be heard. With this instrument a sound can be heard by normal ears in almost perfect stillness at a distance of 15 or 16 meters. Tone or note-deafness is discussed, and the conclusion is drawn that such cases of deafness (deafness to intervals of a whole tone or more) are very rare, but that some well authenticated instances have been recorded. F. T.

Ueber das Verhalten von Vocalen und Diphthongen in gesprochenen Worten. Untersuchung mit dem Sprachzeichner. WM. MARTENS. Zeitschrift f. Biologie, Bd. XXV, H. 3.

This research is in a sense supplementary to that of Wendeler on the consonants, executed with the same instrument. (See notice of the instrument, AMER. JOUR. PSY. Vol. I, p. 315.) The author had such phrases as *Vater und Mutter*, *Der Donner rollt*, *Mein kleines Kind*, etc., spoken into the instrument, and compared the tracings of the vowels with those taken at the same time from a rapid tuning-fork. He found, like Wendeler, very great irregularity in the curves as compared with those of the vowels when sung; indeed, such great irregularity that no detailed results could be reached. The changes of pitch in the same enunciation were apparently without rule as to amount (the largest were an octave or more), direction, rate, or position on the curve. This great variability explains the difficulty of judging the pitch of the vowel tones and of reproducing them artificially. It may be an advantage acoustically, however, for it tends to obscure all tones except the over-tones near to the resonance-tone of the cavity of the mouth, and on these the recognition of the vowel depends. Changes in that cavity itself may also influence the form of the tracing. Experiments with a siren showed that the ear still perceives a *tone* rather than a noise when the pitch of a note is run down through an interval as great as that common in the vowel variations. The speaking voice seemed to be keyed lower than the singing voice, so far as tests could be made. The average length of a vowel tone was 0.182 sec., minimum 0.038, maximum 0.549. In the diphthongs the voice passes from one vowel to the other through a transitional stage that can be seen in the best tracings. When care is not taken in pronunciation the second vowel is apt to be slighted. The article is followed by full numerical tables and a plate giving some of the tracings and also plotted curves for the variations of vowel tones.

Second Report on Experimental Psychology: upon the Diagram Tests. Prof. C. S. MINOT. Reprint from Vol. I, No. 4, Proceedings Am. Soc. for Psych. Research, 1889.

The diagrams here tabulated were drawn in response to the following request: Please draw ten diagrams on this card, without receiving any suggestions from any other person, and add your name and address. Five hundred and one sets were received, 310 from men, 169 from women, and 22 without names. Eighty-three different figures are tabulated. The following are the first ten in the table of frequency given; the first number after each is the number of times it occurred; the one in parenthesis is the number of cards upon which the figure was found, some of the cards showing the same figure more than once: Circles, 209 (202, or 40 per cent); squares, 174 (168, or 34 per cent); equilateral triangles, 160 (153, or 31 per cent); crosses, 160 (124, or 25 per cent); letters, 82 (40); diamonds, 80 (79); horizontal oblongs, 78 (78); circles with inscribed figures, 78 (64); stars, 77 (65); faces with profile to the left, 61 (47). Classifying by larger groups, there were 287 circles of all kinds, 236 squares, 220 triangles, 245 four-sided figures, 149 other straight-sided figures—together more than one-fifth of the whole. The average places in the order of drawing (assuming that the figures were drawn from left to right as in writing), for the ten figures nearest the first are as follows: equilateral triangles, 2.6; squares, 3.2; right-angled triangles with hypothenuse left, 3.8; circles, 3.9; right-angled triangles with hypothenuse right, 4.1; faces not in profile, 4.4; faces profile to the right, 4.5; diamonds, 4.6; horizontal oblongs, 4.6; faces with profile left, 4.7. The figures drawn by the men showed a good deal more variety than those by the women. Some of the unusual diagrams are to be accounted for by the respondents having drawn what they habitually draw when scribbling; others by professional associations. Some were probably suggested by objects present, though most seem to have been of subjective origin. Ease of execution was generally a controlling factor. Like the association experiments of Cattell and Bryant below, these show the extent to which individual minds run in common and well-worn channels, and emphasize the error of supposing that a simple application of the calculus of probabilities fits such tests as those of thought-transference.

Favorite Numbers. ALBERT WILLIAMS, JR. Scientific American Supplement, March 16, 1889.

Another example of the number habit is here drawn from the United States Census of 1880. The ignorant who have to guess at their own ages, or to have some one else guess for them, are very likely to be set down in accordance with such a habit. The following are the numbers of persons between the ages of 28 and 42 reported for Alabama, where the negro population raises the percentage of ignorance, for Michigan, and for the whole country, given in thousands and tenths:

Age.	Ala.	Mich.	U. S.	Age.	Ala.	Mich.	U. S.
28	19.2	30.0	850.0	32	12.4	24.4	654.8
29	11.2	23.1	621.8	33	10.6	21.9	580.9
30	30.9	32.5	1,094.3	34	10.0	21.0	546.2
31	8.4	18.9	492.5	35	22.3	26.3	871.0

Age.	Ala.	Mich.	U. S.
36	10.5	21.8	581.6
37	8.7	19.2	495.1
38	11.3	21.3	594.5
39	7.3	17.7	458.0
40	23.2	26.0	922.6
41	4.6	12.6	323.6
42	6.8	17.5	458.9

There is an evident preference for the tens and fives, and one somewhat less marked for the even numbers, though those next the fives suffer by the greater attractiveness of the latter.

On the Nature of the Knee-jerk. WARREN P. LOMBARD, M. D. Reprint from the Journal of Physiology, Vol. X, Nos. 1 and 2.

Of the two opposing theories of the knee-jerk (that it is a direct result of the twitch to the quadriceps muscle, and that it is a full reflex process), the neat experiments of Dr. Lombard strongly support the second. The first has to assume what has yet to be proved, namely, a continuity of muscle-tonus and a dependence of the irritability of the muscle on its tension. The knee-jerk can apparently be present or absent without reference to the presence or absence of tonus, and artificial tension does not restore a lost knee-jerk. It can vary in amount independently of small variations in tension, and vary more rapidly than the irritability of the muscle. The first theory meets a difficulty in the reinforced knee-jerk because moderate reinforcing acts do not change the tension nor the irritability, and another difficulty in the fact, discovered by Mitchell and Lewis, that contractions produced by electrical stimulation cannot be reinforced. Moreover, not only the extensors, but occasionally the flexors also, respond to the stimulating blow—a fact not to be explained by direct stimulation.

A Contribution to the Study of Muscular Tremor. FREDERICK PETERSON, M. D. Reprint from Jour. Nerv. and Ment. Disease, Feb. 1889.

This contribution is in the nature of a preliminary study, intended rather to demonstrate a method than to present results. Twenty-five myograms taken in various nervous diseases with an Edwards sphygmograph, which the author recommends for such purposes, are given. The rates of tremor in most cases suffer some modification by the will of the patient, but they may be divided into two groups; one rapid and not far from 10 per second (the normal innervation rate according to Horsley and Schäfer), the other about half that rate. It would seem, therefore, that these diseases in some way make the muscle responsive to single impulses of innervation, or to groups of two or more. The myogram of the tremor of paralysis agitans has been shown by others to be dicrotic. Though recognizing the need of further investigation, the author is inclined to regard all except fibrillary tremors as of central origin.

Das Kopfschwingen. J. RICH. EWALD. Pflüger's Archiv, Bd. XLIV, H. 7-8-9.

By *Kopfschwingen* Prof. Ewald means the rapid from-side-to-side vibration of the head that can be voluntarily produced by taking a full breath and tensing up the muscles of the neck. With practice, the vibrations can be executed with those muscles alone, and give graphic tracings almost as uniform as those of a tuning-fork, (for methods of registration, the original should be consulted). The average rate for the first second was 15–16, never more than 17 ; in a few persons of more than fifty years, less than 14½. In the same person it was constant, varying for Ewald himself only about half a vibration from the average. It could not be voluntarily changed, was not influenced by practice, and did not depend on the inertia of the head. The vibrations could not be kept up longer than 5–7 seconds, and grew slightly slower after the first second. They differ from tremors in regularity and constancy and in ceasing immediately when the head is fixed, which the fatigue tremor of the same muscles does not do. The author's explanation is that, by the contraction of one muscle and the accompanying movement, its antagonist is stretched, and the stretching makes it more excitable either directly, or reflexly as alleged for the knee-jerk. In trying to get tracings from the somewhat similar motions of the jaw when the teeth chatter, he failed because the attempt always inhibited the motions.

Ricerche di psicologia sperimentale. Prof. G. SERGI. Reprint from the Rivista sperim. di Freniat., Vol. XII, fasc. 4, 1886.

Professor Sergi here reinsists on an objection made by him to Wundt's conception of the central stages (perception, apperception and volition) in simple reaction-times, believing that under the usual circumstances of experiment the processes are not separable. The criticism does not, however, hold with full force against Wundt's present views, published since the appearance of Sergi's article ; for in the last edition of his psychology he gives very great prominence to a shortened form of reaction in which the central processes enter but partially or not at all, and, like Sergi, speaks of it as purely reflex. Wundt's pupils have found this form of reaction with light, sound and electrical stimulation. Sergi reports it for sound only, but gives smaller values to it, and finds it under circumstances not admitted by Wundt. With his best subject he gets the following reaction-times :

	Uncorrected mean. s.	Corrected mean. s.	Maximum. s.	Minimum. s.
With signal,	0.070	0.0644	0.093	0.047
Without signal,	0.0577	0.0492	0.0655	0.034
" "	0.0725	0.067	0.0765	0.059
Barely perceptible sound,	0.0744	0.0688	0.083	0.054

In making the corrected means in the first and third series a single reaction from each was alone excluded ; the maxima and minima seem to be for the corrected series. The values given by other experimenters for sound reactions range from 0.11 to 0.18.

Sergi also reaches results flatly contradicting two most widely accepted generalizations of psychometry, namely, that reaction-times with a premonitory signal are a good deal shorter than those without, and that reactions to strong stimuli are quicker than those to faint ones. In the table above there is no evidence of either. With other normal subjects he got for reactions without signal, series that ranged from 0.07 to 0.11, all as low or lower than those usually found with a signal ; and with the faint sound, on the only other subject reported, he got an uncorrected mean of 0.097. The stimulus was a bell stroke, and the time was taken with the chronoscope. The explanation of these results requires no more than an extension, to those cases where there are no aids to concentration, of the idea that concentrated attention can voluntarily set up a brain-reflex; and Sergi seems to have been the first to suggest that idea. His results stand, however, so directly in opposition to previous results that skepticism is natural. It may be asked, for example, whether the starting of the chronoscope itself did not act as a signal; and some chronographs also run slower for as much as three-quarters of a second after the throwing in of the hands (see C. S. Peirce, U. S. Coast Survey Reports for 1870, Appendix 21, p. 212). The subject is one worth further investigation. Wundt strangely seems not to have known of this paper when preparing the new edition of his psychology. E. C. S.

Recherches expérimentales sur la durée des actes psychiques les plus simples et sur la vitesse des courants nerveux à l'état normal et à l'état pathologique. A. RÉMOND. Paris: Octave Doin. 1888, pp. 135.

The author of this thick pamphlet has made 30,000 observations in all, on students, soldiers, old people, hemiplegics, and those suffering from myelitis, general paresis, epilepsy, hallucinations, delusions, hysteria, and muscular atrophy. He has plenty of "results" too, but unfortunately not of the decisive and convincing kind that might be hoped. The apparatus used was poor; the chronoscope (*chronomètre de d'Arsonval*) must have been far inferior to the Hipp instrument, and the other arrangements, if fully described, were not up to modern requirements. The tables, though numerous, show only the maxima, minima and averages of the series, and not the average variations. The stimulus was tactile; a certain portion of the hand, shoulder, ankle and hip were touched with a ball, a conical point and a needle. Discrimination-times were measured by using the ball and point in irregular order and requiring reaction to only one. The general conclusions reached are given for what they are worth. (1) The approximate speed of transmission in the sensory nerves in normal subjects is about 34.72 m. per sec. in the legs and 27.02 m. in the arms (determined by Schelske's method, *i. e.* by comparing reaction times for stimulated points unequally distant from the brain). The rate seemed increased in those of the aged affected with general atheroma, in hemiplegics (on the diseased side when there was secondary contracture), and in delirium of persecution. In all other cases the rate was diminished. (2) Motor conduction in the cord and nerves is quicker than normal in flaccid hemiplegia and in the presence of a disturbing noise, about normal with the epileptics, and slower with the other subjects. (3) The discrimination-time, normally 0.063—0.070 sec., is longer with a disturbing noise and for all the invalid subjects except the epileptics (with whom it is shorter), the hemiplegics with contracture when stimulated on the sound side, and the simply aged, with whom it is normal. (4) The simple reaction-time, found in the normal to be 0.1545—0.1587 sec., is shorter after taking phenacetine or antipyrine, and in old age with general atheroma, hemiplegia (on the sound side; on the diseased side likewise with contractures), and in hysteria. (5) The reaction-time is regularly shorter for strong stimuli, except sometimes with the aged, hemiplegics, epileptics and those with myelitis. (6) The psychic stage is the largest of those that make up the simple reaction-time, except with the epileptics, where it is exceeded by that of centrifugal conduction.

Mental Association investigated by Experiment. Prof. J. McK. Cattell and S. Bryant, D. Sc. Mind, April, 1889.

These experiments touch the question at two points: the time required, and the relative frequency of the different kinds. Three methods were used: one in which a printed word was shown, the word suggested called out, and the time accurately measured with apparatus; another, in which lists of ten words were shown, the word suggested by each called out, and the time for one found from the time for all; and a third, in which a word was pronounced and the subjects wrote down the words successively occurring to them in 20 seconds. None of these measure the bare process of association, and the second and third take in other processes; still it is possible by them to reach relative results of some certainty and to gather interesting statistics. The first method, used by Cattell and Berger, gave average association times of about half a second. The second method was used by the experimenters on themselves with very many words, and, with fewer, on a number of other people, including university graduates, students in a ladies' college and in a German gymnasium. The times found vary from 1.14 to 7.07 secs., the first for Cattell himself with concrete nouns, the second for the youngest boys tested at the gymnasium and with abstract nouns. The third was applied chiefly with London and Dublin school-girls. The oldest and most advanced averaged one association in 4.13 seconds, the youngest one in 9.33. Of the twenty nouns used, the English girls gave the greatest number in 20 seconds for "ship" (4.8), the least for "virtue" (2.3). The different series show that the association time is longer for abstract than for concrete nouns, and that maturity and mental discipline tend to reduce this difference.

Of fresher interest are the statistics of frequency. The first table, based on the associations of 465 persons with 10 concrete and 10 abstract nouns, gives the associations occurring ten times or over. The table averages less than eight associations per word, and yet contains more than half of all given—an evidence of the general uniformity of mental action in different individuals. All the associations with the words "house" and "time" were classified after a scheme like Wundt's, and showed, among other things, that most of the associations of co-existence were visual, like "house" and "garden"; most of those of succession were verbal, like "house" and "house-top." Treating in the same way a mass of over 12,000 observations from 512 persons, school-girls predominating, it appears that concrete nouns owe their associations a little more frequently to connection in sensation than to logical connection; that to go from whole to part, or to specialize, is much commoner than to go from part to whole, or to generalize; also that it is easier to go forward ("house" to "house-top") than backward ("house" to "glass house"), and easier to go to final than to efficient causes. The associations of abstract nouns, except verbal associations, are rarely due to the senses; about two thirds of the cases were correlations and specializations. Classifying again for observers, it appeared that those that write and teach prefer logical and verbal associations; two teachers gave respectively 33 and 26 per cent of verbal associations against about 1 per cent for their pupils; the favorite form for the latter was from whole to part. A further table shows the influence of the original word used, "tree," for example, suggesting "leaves" (whole to part), while "courage" suggested its like or its opposite. By no means the least interesting part of the research, and one contributing greatly to the proper conception of the results, is the appendix of subjective observations by the experimenters and several of their subjects.

(1). *Untersuchung über die Latenzdauer der Muskelzuckung in ihrer Abhängigkeit von verschiedenen Variabeln.* Dr. ROBERT TIGERSTEDT. Archiv für Anat. u. Physiol. (Physiol. Abth.), 1885, p. 111.

(2). *Die Scheinbare Latenzperiode der Erregung bei directer Muskelreizung.* Dr. EMERICH NAGY V. REGÉCZY. Pflüger's Arch., Vol. 43, p. 584.

(3). *On the Normal Duration and Significance of the "Latent Period of Excitation" in Muscle-contraction.* G. F. YEO. Jour. of Phys., Vol. IX, p. 396.

Helmholtz in 1850 discovered that a period of time elapses between the moment of stimulating a muscle and the commencement of the ensuing contraction. He found this latent period to be approximately 0.01″, and thought that submaximal shocks, as well as loading, overloading and fatigue, caused this value to vary. He did not attempt to explain the meaning of this lost time, and even thought it possible that contraction might begin immediately on stimulation. Since 1850 other investigators have variously estimated the latent periods within very wide limits; but after the researches of von Bezold and Bernstein, a duration of 0.01″ — 0.02″ seems to have been universally accepted as the normal period, coupled generally with the view that this time is consumed by certain molecular changes prerequisite to contraction.

In 1879, however, Gad combated this opinion, and was led by Bernstein's work, which showed the latency of the electric changes of the muscle preceding contraction to be about 0.001″, to regard the latency of the individual muscle elements as of similar amount. He pierced the belly of the muscle with a recording lever, and, upon stimulating the lower end, obtained a curve indicating an initial lengthening of the muscle before contraction began. He considered this as evidence that when the muscle is stimulated, the muscle elements originate a local contraction and stretch their fellows more remote. According to his view, therefore, the latent period is the time required for the contraction to include a number of muscle elements more than sufficient to compensate the initial elongation. Since the whole must be greater than any of its parts, he maintains that the latency of the individual elements must be less than that of the whole muscle.

(1). In *du Bois-Reymond's Archiv* for 1885, Robert Tigerstedt gave the results of his research upon the latent period. As had been conceded before him, he found that temperature was an important condition, and concluded that the latency from 12° — 16°.9 C. is 0.006″; from 17° — 18°.9, 0.005″; and from 20° — 29° C., 0.004.″ Bernstein had discovered that the impulse from nerve to muscle was delayed in the nerve endings 0.0032″. Tigerstedt, who adopts 0.002″ instead of 0.0032″, notes that in direct maximal stimulation of an uncurarized muscle, in consequence of this delay, there are two waves of excitation—one proceeding from the muscle substance directly, and another from the nerve endings, and these waves may summate. If curare is not administered, the latency is longer and maximal contractions are at times obtained with much weaker shocks; but if the endings are thrown out of action by curare, the latency is not dependent upon the shock. The latency within normal limits is independent of the tension of the muscle.

Taking up the line of thought pursued by Gad, he declares that many muscle elements must be active before a contraction becomes manifest, and that without doubt the latent period of these elements is less than that of the whole muscle. The latent period of the muscle, according to him, is the time taken up by the wave of contraction, which begins at once, or very soon after the stimulation, to traverse a sufficient length of muscle (12 mm.) to render the contraction perceptible.

(2). In his article in *Pflüger's Archiv*, Vol. 43, Regéczy seems to have accepted the work of Gad and Tigerstedt without qualification, and boldly speaks of the "apparent" latent period. He finds that the muscle curve generally rises earlier if the excitation starts from the lower end than if from the upper one, and that the duration of the latency varies inversely as the thickness and length of the muscle. When the stimulation falls in simultaneously at both ends of the muscle, the latency is longer than when the middle of the muscle is stimulated, because more resistance and more stretching are encountered. In short, he believes that all conditions influencing the period begin in affecting the stretching, and were it not for this, no latency of the excitation would exist.

(3). To this view of Gad, Tigerstedt, and Regéczy, Yeo takes exception. He maintains that it not only disagrees with all that we know of contractile tissue, both animal and vegetable, but also that it has not been definitely proved. Concerning the latent period, however, he admits that the old estimate of 0.01″ is certainly too high, and thinks that 0.0065″ is more accurate. In general, his results seem utterly at variance with those of the three investigators just mentioned. In the first place, contrary to Tigerstedt's view, he declares the latency to be prolonged by increase of load; and in regard to stimulation he says, "It would seem that the latency varies with changes of intensity of stimulation, even when the nerve terminals are paralysed [by curare] and the stimulations are maximal." He finds Gad's curves are very easy to get, not only as Gad did, by stimulation of the lower end, but also by stimulating the muscle throughout its whole length, and that with any direction of current; by using the electric signal for marking the beginning of the contraction, he affirms that the elongation occupies the second half of the latent period of the half muscle, and hence cannot, as Gad supposed, be the cause of the latent period. He concludes from his work upon this point, that a preliminary elongation does take place at times, when the latency is recorded by a lever transfixing the belly of the muscle, but that it might be due to the non-recognition of the earliest contracting efforts, while the heavy direct weight quickly indicates increased extensibility of the tissue; or it might be "that the transfixing needle and electrodes cause some polarisation, and consequent slight tonic contraction, which may be inhibited on stimulation." Again, if Tigerstedt's view be right, viz. that the latent period is the time required for the wave of contraction, which starts immediately after stimulation, to travel over a certain distance before the contraction is able to show itself, then the latency of the part of the muscle where the shock is sent in should be little or nil; yet Yeo obtained nearly the typical latency under such circumstances. The author concludes that "the latency of the individual muscle elements is a theoretical speculation which appears difficult to determine by experimental methods, and which I feel disinclined to investigate. Graphically I think it cannot be shown to be shorter than that observed at the actual point of stimulation—*i. e.* nearly .005″."

What is wanted is plainly more work, and such work as will fill up the gaps in the researches now extant. Thus, to mention some of the more obvious defects, Gad does little more than state his results; Tigerstedt, generally very satisfactory and explicit, fails to give the name of the muscle used by him, although it is known that the degree of regularity of form of the muscle is of considerable importance; and Yeo, while finding fault with Tigerstedt for employing hypermaximal and injurious stimuli, seems possibly himself at times to have made use of submaximal shocks.

It is interesting to compare the various numerical values of the latent period found from time to time by different observers. The excellent table of Yeo is used as a basis for the following:

1850.	Helmholtz,	.01″	1877.	Lautenbach,	.008″
1859.	Harless,	.0187		Brücke,	.007
	Bezold,	.0136		Gad,	.004
	Wundt,	.01	1879.	Sewall,	.01
1862.	Fick,	.007		Richet,	.008
1867.	Place,	.005		Langendorf,	.009
1868.	Marey,	.01		Mendelssohn,	.008
	Klunder and Hensen,	.0085	1883.	Cash and Yeo,	.009
	Lamansky,	.0075		Rosenthal,	.009
1870.	Volkmann,	.01	1885.	Fredericq,	.018
1871.	Valentine,	.021		Tigerstedt,	.005
	Bernstein,	.0188	1888.	Regéczy,	.0033
1874.	Ranvier,	.015		Yeo,	.0065

EDW. C. APPLEGARTH.

Die Theorie der Muskelcontraktion. G. E. MÜLLER. Nachrichten von der Königlichen Gesellschaft der Wissenschaften und der Georg-Augusts-Universität zu Göttingen, No. 7, March, 1889.

As the whole question of the latent period is intimately connected with the obscure problem of muscle contraction, perhaps the elaborate theory of contraction recently formulated by Müller may be of interest here.

This author, discarding the views of Krause, Merkel, and Engelmann, starts out with three fundamental, constituent elements of the muscle fiber, which he designates *disdiaklasten*, *gerüstsubstanz*, and *muskelsaft*. The disdiaklasts (Brücke's term for the doubly refracting particles of the muscle) are here elongated aggregates of such particles or micellae, so arranged that their long axes are parallel to that of the fiber. Running from one end of the fiber to the other there are series of cross columns of these disdiaklasts at the level of the anisotropic bands, the individual disdiaklasts of each column being united laterally with their neighbors by little crosswise rods or threads (*querbälkchen*) forming a network, and longitudinally, that is across the isotropic bands, with the corresponding disdiaklasts of the succeeding cross column by somewhat similar little rods of lengthwise disposition (*längsbälkchen*). The series of disdiaklasts so joined together by the *längsbälkchen* are the fibrils, and these together with the network of *querbälkchen* constitute the *gerüstsubstanz*. All the remaining space within the sarcolemma is filled with *muskelsaft* or muscle plasma, and it is in this substance that the heat production takes place during activity of the muscle.

He supposes that contraction is some such process as this. By anabolism, a store of energy is laid up, which is partly converted into heat upon stimulation of the muscle. Owing to their nature and form considered crystallographically, the disdiaklasts are pyro-electric, and from their arrangement in the fiber the neighboring ends of the disdiaklasts of consecutive cross columns will contain electricity of opposite signs, the effect of which will be to pull the elements from a longitudinal to a transverse position. This tendency, however, will be resisted chiefly by the *gerüstsubstanz*, but also by the sarcolemma, the perimysium, etc. To the opposition so engendered he applies the name *inner-contraction-resistance*. The cross and lengthwise rods differ in structure microscopically. The lengthwise rods are firmer and less capable of swelling than the crosswise rods, which are full of so-called *functional pores*, whose duty it is to absorb the surrounding plasma. When the crosswise rods are stretched lengthwise, as is the case when the disdiaklasts are forced into their transverse position, their capacity for the plasma is increased, so that the muscle on contracting shortens and swells, not simply on account of the change of position, but also because the *querbälkchen* become very turgid. Now when the electric charge of the disdiaklasts sinks to a certain limit, the retention of plasma, in consequence, is discontinued, but the process of expulsion is comparatively slow, due to inadequate elasticity. The return of the disdiaklasts to their normal position being thus retarded, the relaxation is somewhat prolonged.

He puts great stress upon the following myothermal law, which is conceived to be fundamental: "The heat production which takes place in consequence of a given stimulus in a particular part of the plasma, is so much less, the higher the pressure is, under which this part of the plasma is." Indeed, as much depends upon the plasma as upon the electric charge of the disdiaklasts. Thus to make an application, he says, the tension which a muscle possesses at any given time depends not simply upon the electric charge then present, and upon the length of the muscle at that moment, but also upon the value of the *functional imbibition pressure*, or the pressure which the plasma in the functional pores supports. Again, when products of muscle activity accumulate, they increase the consistency of the plasma, which then opposes any change of form of the muscle, and at the same time lessens the rate of transmission of the excitation. The advantage of the circulation is, then, in maintaining the low consistency. In another place, speaking of the plasma, he says, through every stimulation the irritable material, or the quantity of the irritable molecules of the plasma, through whose decomposition the production of heat is effected, is lessened according to the measure of intensity of the excitation. This serves naturally to weaken the influence of the following stimulus.

Müller applies his theory very deftly, and the whole thing possesses a degree of plausibility, but still there are no facts as yet to substantiate it, and in the absence of these it is well to await developments, especially as he gives this paper simply as a preliminary communication. E. C. A.

III.—ABNORMAL.

A FEW PRACTICAL SUGGESTIONS TO PHYSICIANS IN ASYLUMS, HOSPITALS, ETC., FOR THE OBSERVATION OF PATIENTS SUFFERING FROM MENTAL OR NERVOUS DISEASE.

I.

The following suggestions are made with the view to getting data beyond those strictly necessary for diagnosis, since such data would be extremely valuable both from the psychological standpoint and as a basis for determining the function of diseased parts, should the case come to autopsy.

How to observe.—Patient should be away from all distractions, in a room apart, and at ease—as a rule, either sitting or lying down, and with the mind placid. Experiments should rarely last an hour, as the attention is easily fatigued. Successive observations should be made at the same time of day. For experiments not involving the eyes it is best to have the patient thoroughly blindfolded.

Records.—May be written, or in some cases, *e. g.* areas of anaesthesia, plotted on an outline of the body, such as may be copied from any anatomy.

In progressive disease, a careful study of one patient has more value than a casual study of several.

Beginning with the skin sensations.—Is the sense of contact anywhere absent? Where? If present, test "discriminative sensibility" with compasses. For a table of the normal discriminations in various regions see Foster's *Text-book of Physiology* under "Tactile Sensations." Compasses should be made of a substance non-conductive of heat, and slightly blunted at the points, like the rounded end of a small needle. The best form is that where one point is fixed and the other slides along an arm (at right angles to the first point) on which a scale is marked so that the distance between the points is easily read off. See *Aesthesiometer*, by J. Jastrow, AMERICAN JOURNAL OF PSYCHOLOGY, Vol. I, p. 552.

Sense of location.—The patient to touch a spot on his body which the observer is touching.

Temperature sense.—Discrimination of differences. Two objects—preferably thermometers with large bulbs—the temperature of which is known, are touched successively to the same spot on the body and the patient required to distinguish between them.

Sensibility to heat and cold.—Test by applying metal points suitably warmed or cooled. If these sensations are dull, the area stimulated must often be large, a square inch or more, to get any reaction at all. Refer to *Eine neue Methode der Temperatursinnprüfung*, Dr. A. Goldscheider, Archiv für Psychiatrie und Nervenkrankheiten, Bd. XVIII, Heft 3, 1887. *Research on the Temperature-sense*, H. H. Donaldson, Mind, No. XXXIX, 1885.

Those cases in which the sensation for one sort of temperature stimulus remains while that for the other is absent, are specially important.

Motion on the skin.—By drawing a point up or down the skin of a limb, to determine whether the direction can be recognized. Refer to *Motor Sensations of the Skin*, by G. Stanley Hall and H. H. Donaldson, Mind, No. XL, 1886.

Pressure.—By placing weights successively on the same spot, the patient to detect the difference between any pair of weights. Such weights can be easily made by loading paper cartridge-shells with various charges of shot.

Tickling.—It is specially important to determine the conditions under which this disappears.

Muscle-sense.—Discrimination of weights. Weights to be lifted and thus distinguished. Mr. Francis Galton has a set of weights for this purpose. See *On Apparatus for Testing the Delicacy of Muscular and other Senses in Different Persons*, by Francis Galton, F. R. S., Journal of the Anthropological Institute, May, 1883. A brief account of this is given in *A Descriptive List of Anthropometric Apparatus, etc.*, published by the Cambridge Scientific Instrument Co., Cambridge, England. Other apparatus, to be mentioned later, is described in the same publication, and I shall refer to this catalogue in these cases simply as the "descriptive list."

With paper cartridge-shells filled with shot, the more elegant apparatus of Galton can be fairly imitated.

Position of limbs.—To imitate with a sound limb the position in which the affected limb is placed, or the reverse. Eyes closed.

Clonus, Knee-jerk. See *The Variations of the Normal Knee-jerk and their Relations to the Activity of the Central Nervous System*, Dr. Warren P. Lombard, AMERICAN JOURNAL OF PSYCHOLOGY, Vol. I, 1887.

Vision.—Ophthalmoscopic data. Pupillary reactions. In case of paralysis of the external ocular muscles, the subjective sensations of motion on innervation of the paralyzed muscles.

Field of vision.

Field for various colors. For this, some sort of a perimeter is needed.

Color blindness. Some system of colored wools is the simplest device for this purpose.

Visualization. Number-forms, etc. See *Inquiries into Human Faculty and its Development*, Francis Galton, F. R. S., Macmillan & Co., 1883.

Hearing.—Limits of audition, by means of a small whistle. See "Descriptive list."

Appreciation of pitch. For specially constructed organ-pipe see "Descriptive list."

Colored sounds. Associations of certain colors with given tones. Refer to *Zwangmässige Lichtempfindungen*, Lehmann and Bleuler. *Inquiries into human faculty*, &c., Francis Galton.

Time sense. Repetition and maintenance of a given tempo. This involves the use of some device by which a graphic record can be obtained—a revolving drum, for example. Refer to a series of articles in *Wundt's Studien* under the title "Zeitsinn."

Smell.—Its delicacy, by means of standard solutions of graded strength.

Taste.—Test different portions of the tongue for bitter, sweet, acid, and salt. For bitter and sweet the test can now be made with accuracy. See *Note on the Specific Energy of the Nerves of Taste*, by W. H. Howell and J. H. Kastle, Studies from the Biological Laboratory of the Johns Hopkins University, Baltimore, Vol. IV, 1887.

Equilibrium sense.—Special susceptibility to dizziness on whirling, etc. These facts bear on the functions of the semi-circular canals. See *The Sense of Dizziness in Deaf-Mutes*, American Journal of Otology, Boston, 1882, by W. James.

Generative organs.—Cremaster reflex. Prepuce: does it cover glans? Perverted sexual instincts; refer to von Krafft-Ebing, *Perversion of the Sexual Instinct; Report of Cases.* (Translated by H. M. Jewett, Alienist and Neurol., St. Louis, 1888, Vol. IX). Dreams; refer to Julius Nelson, *A Study of Dreams*, American Journal of Psychology, Vol. I, No. 3.

Reaction time.—To get valuable results, some apparatus is needed. The simplest is that described by Joseph W. Warren, M. D., in a paper *On the Effect of Pure Alcohol on the Reaction Time, with a Description of a New Chronoscope*, Journal of Physiology, Vol. VIII, 1887. The "Hipp Chronoscope" is a somewhat costly instrument, to be used only with great caution. For the conditions attending its use see *Psychometrische Untersuchungen*, by J. McK. Cattell, Philosophische Studien, edited by Wundt, Vol. III, 1886. There is further a way of recording the reaction time on a revolving drum, such as is used in astronomical observatories.

Aphasia.—See *Aphasia*, by Dr. M. Allen Starr, Transactions of the Congress of American Physicians and Surgeons, Vol. I, 1888; and Kussmaul, in Ziemssen's Cyclopaedia, XIV, pp. 581–875. Refer to *Versuch einer Darstellung unserer heutigen Kenntnisse in der Lehre von der Aphasie*, Ernst Malachowski, Volkmann's Klin. Vorträge, No. 324.

Handwriting.—Refer to *Die Schrift*, von Dr. Albrecht Erlenmeyer, Stuttgart, 1879. *On the Writing of the Insane*, by G. Mackenzie Bacon, M. D., London, 1870.

Muscles.—Exact determination of muscles affected in paralysis. Very important for getting the motor centers in the spinal cord.

II.

Mental derangement.—Study of the incipient stages and the central point of the alienation. See *Insistent and Fixed Ideas*, Edward Cowles, M. D., Am. Journal of Psychology, Vol. I. *Paranoia*, William Noyes, M. D., Am. Journal of Psychology, Vol. I. *Extract from the Autobiography of a Paranoiac*, Dr. Frederick Peterson, Am. Journal of Psychology, Vol. II, No. 2, 1889.

III.

Removal of central nervous system.—Dura to be left at points where it is at all adherent. If the brain is to be used for microscopic examination, injection of it through the aorta with 2 per cent bichromate of potash removes blood and facilitates hardening; not permissible, of course, where hemorrhage is suspected.

The cord can be removed from the ventral side by chipping off the bodies of the vertebrae with broad chisels specially made for the purpose, or dorsally, by sawing through the arches of the vertebrae with a double saw (made by Luer, Paris), or by the common chisel. The spinal ganglia should be removed whenever possible; also sympathetic ganglia. Bits of peripheral nerve should be tied out straight on a bit of wood or cork before being put in the hardening fluid. Refer to v. Bischoff's *Führer bei den Präparirübungen*, etc., edited by N. Rüdinger, München, 1886.

The form of the brain is best preserved by receiving it in a jelly mould padded with moist cotton; by the use of two such moulds it can be turned over with little distortion.

In taking the weight it should be noted what membranes are on; how much of the cord is attached; whether the fluid has been removed from the ventricles, etc.

For the convolutions Ecker is the most desirable authority, having been accepted in the current text-books. Refer to *Die Hirnwindungen des Menschen*, etc., von Alex. Ecker, zweite Auflage, Braunschweig, 1883. There is a translation of the above (1st edition) by R. T. Edes, New York, 1873.

In sectioning the brain.—First, open the lateral ventricles by a median section a few millimeters to one side of the middle line. This empties the ventricles of fluid. All cuts to be made with one sweep of the knife, giving a smooth surface. In the case of the hemispheres, cut from the middle line outwards, thus leaving the pieces bound together by the pia in such a manner that after the sectioning is over the convolutions can be reconstructed. For details, see *Die Sections-Technik*, etc., von Rudolf Virchow, dritte Auflage, Berlin, 1884.

The records of lesions are made much clearer by a sketch. Lebon & Co., 23 Southampton Buildings, Chancery Lane, London, W. C., England, furnish sets of typical sections of the brain printed on gummed paper. On these the lesion can be indicated and the sketch then pasted in the record book.

Preserving material.—The brain and cord are best hardened for most purposes in potassium bichromate, 2 per cent. The quantity must be large at first and often changed. A brain should not be put in less than one gallon of bichromate, and this should be changed as often as it becomes turbid, say 6–10 times. During hardening in bichromate the specimen is best kept at a moderately low temperature, say 40°–50° F., and in the dark. The strength of the solution may be gradually increased to 4 or 5 per cent in the later stages of the process. When the material is hardened it is firm yet springy, and cuts in thin sections with a razor without having the sections break. The length of time requisite for this ideal condition varies with the part of the nervous system, the strength and quantity of the bichromate, the freshness of the specimen, and the temperature. For an entire brain it may vary from 6 weeks to 3 months or more. Where the material is soft through post-mortem changes, hardening can be obtained by adding ½ per cent copper sulphate to the bichromate solution. The specimen being hardened, it must be thoroughly washed out in cold running water. This may take 3 or 4 days in the case of an entire brain. The specimen may then be preserved indefinitely in strong camphor water or 80 per cent alcohol. In camphor water it should be watched, to make sure that no mould forms on it. Brains preserved in camphor water are to be preferred for further microscopic study.

Refer for description of the finer anatomy to *Anleitung beim Studium des Baues der nervösen Centralorgane im gesunden und kranken Zustande*, von Dr. H. Obersteiner, 1888. An English translation of this book is promised soon.

In addition to the books and articles mentioned above, the following may be referred to as embodying recent advances in the field of physiological psychology and as illustrating the value of experimental and clinical data to psychology: *Grundzüge der physiologischen Psychologie*. W. Wundt. 2 vols. Leipzig, 1887.—*Elements of Physiological Psychology*. Geo. T. Ladd. New York, 1887.—*La Psychologie physiologique*. G. Sergi. Paris (a translation from the Italian).—The following monographs of Ribot's (F. Alcan, Paris) are popular, but are to be recommended for a general view of the subjects treated: *Les maladies de la mémoire; Les maladies de la volonté; Les maladies de la personnalité; La psychologie de l'attention; La psychologie allemande contemporaine*. Most of these have been translated; the first forms one of the International Scientific Series; the next two are, I believe, in the "Humboldt Library," and the last is published by the Scribners under the title of "German Psychology of To-day."

It would yield the best results if any one interested in work of this nature would settle on some single topic and pursue that specially. As the above lines are merely suggestions, the author will most gladly answer any further questions that may arise regarding methods or apparatus. Pathological nervous material which may be consigned to the author will be examined and reported on with all due promptness. HENRY H. DONALDSON.

CLARK UNIVERSITY, *June*, 1889.

Ueber optische Aphasia und Seelenblindheit. C. S. FREUND. Archiv f. Psychiatrie, Bd. XX, H. 1 und 2.

The connection of aphasia with disturbances in the visual centers has been noticed by several observers, and Wilbrand has touched on the probable explanation. Dr. Freund's object is to show that this "optical aphasia" is a distinct and independent kind. The seven cases which he gives (two of his own observation and five from the literature), allow the following general description. All showed cerebral defects of vision; in 4 right hemianopsia was demonstrated, in 1 (left-handed) left hemianopsia; and similar trouble was doubtless present in the other two. In five there was psychic blindness. Satisfactory tests for agraphia and alexia were wanting in most. The distinctive disturbances of speech were in all more or less clearly connected with the visual defects, the differences coming chiefly from the degree in which the "optical aphasia" was overlaid by the other kinds. In the simplest cases there was loss of nouns (their place being frequently taken by circumlocutions) and of the ability to name objects. At least two of the cases were helped by handling what they were to name; and to one words not to be given at request sometimes recurred spontaneously. The results of section, in the six cases in which it was made, show extensive lesions, generally in the occipital and tempero-occipital lobes. Bilateral lesions were associated with psychic blindness, unilateral not.

The connection of these lesions with the language symptoms and others is made clearer by reference to a schema. It is to be supposed that the two visual centers in the left and right occipital lobes are connected by lines of fibers with the speech centers in the left temporal and frontal lobes, and by other lines, in which half of each retina is represented, with the eyes. Of the many possible lesions of these centers and tracts, three are of interest in this connection, namely, (1) the cutting of the fibers between the left visual center and the eyes and of those between the right visual center and the speech centers; (2) destruction of both visual centers; and (3) destruction of the left visual center and cutting of the fibers between the right visual center and the speech centers. (1) The first would represent the purest cases of "optical aphasia." The cutting of the first set of fibers would produce right hemianopsia and prevent the left visual center from receiving new visual impressions. Its connection with the speech centers would, however, still be in function and its optical memory images available for speech. The cutting of the second set of fibers would prevent the use in language of the images belonging to the right center. Now for undisturbed speech it is necessary not only that the sensory and motor images of the word, but also that those that form the concept of the thing for which the word stands, and the connections of the two sets, be intact. In spontaneous speaking there would be temporary difficulty in finding the nouns corresponding to new visual images (recorded only on the right side) or to the sense impressions of the instant. The patient would be unable to name objects shown him, though he could point them out when named, because the uninjured connections of the visual center with the other sensory centers would enable him to form full and concrete concepts of objects. In the same way naming would be facilitated by handling the objects, and there would be no psychic blindness. (2) The symptoms of the second kind of lesion would be total psychic blindness. The concepts would lack all their visual part, would be less concrete, the course of thought would thus be disturbed and consequently speech, while all the time the speech centers proper remained intact. Touch would give no help here. (3) The third kind of lesion would show right hemianopsia, non-recognition of objects formerly represented only in the left visual center, *i. e.* right-sided psychic blindness. Those represented in the right visual center could be recognized, but not named. Objects named could be pointed out and the patient would be helped by touch in naming, as in the first case.

The author concludes his paper with a general consideration of psychic blindness (of which he records an additional case in the first part of the paper), pointing out among other things that psychic blindness may result from a cutting of all the association fibers of the visual centers even when the cortex and its connection with the eyes are uninjured. Freund's cases greatly resemble a case reported in the *Neurolog. Centralbl.* No. 17, 1888 (see abstract, AM. JOUR. PSY. Vol. II, p. 175), by Bruns and Stölting, who came to very similar conclusions as regards the explanation of the disturbance. In a review in the same periodical, No. 4, 1889, Bruns brings out the points of divergence.

Ein Fall von Seelenblindheit. Dr. LISSAUER. 48. Sitzung des Vereins ostdeutscher Irrenärzte, Nov. 1888. Allg. Zeitschr. f. Psychiatrie, Bd. XLVI, H. 1.

This case is very like those of Freund. The patient was an old man, with no previous trouble beyond attacks of dizziness and senile weakness of memory. On examination he showed no aphasia nor symptoms of paralysis; right hemianopsia; sight in the center of the field preserved, and amounting to ½ to ⅓ normal; inability to recognize the significance of objects shown or to name them, except with aid of other senses, *e. g.*, touch or hearing; alexia without agraphia; ability to draw outlines of simple objects, but without recognition. The author distinguishes two kinds of psychic blindness, an apperceptive and an associative, caused respectively by injury to the visual centers or to their associative fibers—in this case, since the patient recognized forms, probably the latter.

Ueber Aphasia. LEOPOLD CARO. Inaug. Dis. Arch. f. klin. Med. Bd. XLIII, 1888. Abstract by Sperling in the Neurol. Centralbl. No. 4, 1889.

As an appendix to the body of his dissertation (in which he presents the doctrine of aphasia and his views on the origin of language), the author reports a carefully studied case, in which, as in one of Grashey's, the aphasia rested on a general decline of mental power —a case of amnesic aphasia. The mental processes of the patient were surprisingly slow; six or seven seconds were required for the recollection of his own name, as much as twenty for seeing and naming objects. He himself said: "When I look at the picture, then I don't know—know what it is; I have to—have to look at it closely first, then I have to—have to think over what it is; then what—what it is called." He took four seconds longer to read capital letters than small, though, as it would seem, the reading of letters in series was not delayed. Some letters, especially capitals, could not be written at dictation and generally were not recognized; the figures above 3 also failed of recognition. What the patient did read he did not at once understand. Some questions elicited no answer, from lack of comprehension, and frequent repetition was required. All sense perceptions were soon forgotten, and smell and touch were dull. As in Grashey's case, the patient was unable to read even short words when obliged to take them letter by letter, because the first was forgotten before the last was recognized; for example, "Ochs" could not be read when written with the letters a good deal separated. He could not tell the number of three pencils when they were some distance apart, nor count the sides of a polygon.

L'Alcoolisme, étude médico-sociale. Dr. E. MONIN. Ouvrage couronné par la Société française de Tempérance et précédé d'une Préface par le Dr. Dujardin-Beaumetz. pp. 308. Paris, Octave Doin, 1889.

The aim of this book is to popularize scientific information on the subject with which it deals, and as a popular book it is readable and valuable. The headings of its ten chapters give its scope: Chronic and Acute Intoxication [ivrognerie, ivresse]; The Diseases of Drinkers; The Extent of Alcoholism, its Pandemic Expansion; Alcohol and the Nervous System; Responsibility of Alcoholics; Action of Different Beverages, Distilled Liquors; Absinth; Common Fermented Beverages; Dipsomania; Preventive and Curative Remedies. Under each of these heads is gathered interesting information. Dr. Monin has little liking for what he regards as impracticable total abstinence. What he hopes to see, and what the French Society of Temperance endeavors to secure, is a hygienic use of pure beverages of the less alcoholic kinds. To this end the author would look to government for help in such preventive measures as the betterment of conditions which force the lower classes to drunkenness, the use of the taxing power in favor of the less harmful liquors, and especially the prevention of poisonous adulterations. The state, however, can do comparatively little; measures of social reform, temperance teaching in the schools, temperance societies, coffee-houses, etc., have an important place. For the cure of confirmed drinkers he favors inebriate asylums as in operation in this country. In the last section of this chapter he points out the general lines of medico-pharmaceutical treatment, and gives prescriptions that have been found advantageous.

Ivrognerie, ses causes et son traitement. Dr. P. J. KOVALEVSKY. Translation into French by Woldemar de Holstein, M. D. Kharkoff, M. F. Sylberberg, 1889, pp. 113.

This little book, by the professor of nervous and mental diseases at the University of Kharkoff, is limited more strictly than the last to the statement of present views of inebriety as a disease. The subject is treated under the heads of Symptoms of Inebriety, Prodromal Period, Chronic Alcoholism, Dipsomania, Predisposing Causes of Inebriety (heredity, sex, age, religion, nationality, climate, disease, etc.), Provocative Causes (psychic and physical traumatism, disease, profession, climate, etc.), and Treatment. The author presents his views with admirable clearness, and seems to have succeeded in being popular without sacrifice of quality or form. He recognizes the necessity of a strong foundation in educated public opinion in dealing with the question practically, and, unlike Dr. Monin, would not be opposed to suppressing the sale of alcohol entirely except for medicine and the arts. Even as medicine he would have it used with caution. This difference comes apparently from his much greater familiarity with American and English studies of the subject. Inebriety in the individual is to be treated as a disease; the author has, however, little faith in the various specifics that have been recommended. The hereditarily disposed should have preventive treatment, moral and medical. Those in whom the disease is established should have the discipline of special hospitals (not jails nor insane asylums), where the necessary physical and moral rebuilding of the man can be carried out. As single items among others, manual training and work in the open air are recommended.

Experimentelle Untersuchungen zur Lehre vom chronischen Alcoholismus. F. STRASSMANN. Eulenberg's Vierteljahrss. f. grichtl. Medicin u. s. w. Abstract by Joseph in Centralblatt f. Physiol. No. 24, 1889.

Twelve dogs were treated by the author to determine the difference in the effects of the chronic alcoholism produced by pure alcohol and by that adulterated with higher members of the series. Practically the only constant results due solely to the alcoholization were chronic catarrh of the stomach and fatty degeneration of the liver. The addition to the spirit of 3 per cent of amyl alcohol increased the disturbances and caused death in less than half the ordinary time; and 1 per cent aggravated single symptoms, not however sufficiently to hasten death.

The Etiology of Dipsomania and Heredity of "Alcoholic Inebriety." LEWIS D. MASON, M. D. Quarterly Journal of Inebriety, Oct., 1888.

Dr. Mason gives the testimony of a large number of physicians, including noted specialists, as to the neuro-psychic degenerations in the offspring of alcoholic parents, and has added tables of 600 cases treated at the Inebriates' Home, Fort Hamilton, N. Y. Of the 600 cases, 265 showed inebriate ancestry, distributed as follows:

Fathers	168	Brothers	16
Mothers	9	Grandfathers	12
Fathers and other relatives	32	Grand-parents	2
		Other relatives	26

Thirty-eight showed insane ancestry. In 501 of the 600 the tendency to inebriety appeared between the ages of 15 and 35, in 294 between 15 and 25. Dipsomania is perhaps individually acquired and may result from traumatism, but is generally inherited; there is therefore justification for the term "inebriate diathesis."

In the same number of the *Journal of Inebriety*, Dr. T. L. Wright gives testimony from the English alienists on the same question, and in a third article is gathered a portion of the replies to a question sent out to physicians by the same journal as to the liability of the descendants of inebriates to inebriety, insanity, phthisis, etc.

Inebriate Asylums and their Work. T. D. CROTHERS, M. D.

This pamphlet is part of a lecture before the Y. M. C. A. at Toronto, by one of the foremost representatives of the disease theory of inebriety. The author sketches briefly the history of that theory, the history and present conditions of asylum work, the classes of patients that come and the plan of treatment. In conclusion he mentions some general principles of management, and points to the future before such institutions.

The Question of Responsibility in Inebriety. T. D. CROTHERS, M. D. Alienist and Neurologist, January, 1889.

Four lines of examination will throw light on the question: (1) as to the periodicity of the drinking spells; (2) the immediate connection of alcohol with the crime, its lack of motive, the manner of its execution; (3) the cause of the inebriety—traumatism, etc.; and (4) heredity. When the indications from these concur, insanity and irresponsibility are tolerably certain. The real test is not knowledge of right and wrong, but power of control, which is often to be determined only by careful study of the case. Inebriety is itself a sign of lack of control. In general, all inebriate criminals are of unsound mind. The limits of responsibility cannot be drawn in such a hazy border-land of insanity, and the burden of proof should rest on those that hold the inebriate to be sane.

Die Heilung der durch Morphiumgenuss verursachten Nervenzerrüttung und Willensschwäche. Dr. Constantin Schmidt. 2. Aufl. pp. 48. Berlin und Neuwied: Heuser, 1888.

Sudden dishabituation has, in Dr. Schmidt's opinion, little to recommend it and very much to condemn it. The attempt to cure by substituting cocain is little better. His own plan is gradual reduction, with moral as well as physical treatment. On the psychic part—the rebuilding of the will and the preservation of self-respect—he lays much stress. His first aim is to reduce the amount to the least on which the patient can endure life, say 2–3 cgm. Relations of the utmost confidence between physician and patient are to be cultivated. Dr. Schmidt would not at once take away the patient's syringe, though he would urge the cessation of self-injection. He would not reduce so rapidly as to produce complete insomnia, and would allow a re-increase of dose when neuralgias, migraine, etc., appear. In the second stage, that of complete dishabituation, cocain is an important help, and stimulants are to be used. After discharge the patient is not to be denied the therapeutic use of morphine, on condition, however, that he never administer it himself. The moral treatment must be prolonged after the physical treatment, and the patient shielded from nervous strain and overwork till returned to complete moral vigor. Dr. Schmidt asserts experience in support of his plan, though he does not give specific cases.

Die Selbstheilung der Morphiumsucht. "Professor Carolus." Berlin, 1889, pp. 15.

This pamphlet is the work of a musician who succeeded in breaking up his own morphine habit, and writes to encourage and point the way for others. His method is the simple one of gradual reduction of the dose, with regular weekly or fortnightly abstinences (which he considers of cardinal importance), carried out each time till the consequences become unbearable, and then relieved by a greatly reduced dose. The cure should be carried out with reports of progress from time to time to the family physician or some other, whose services will eventually be needed. A number of points of helpful physical and moral regimen are also mentioned. Such a cure would be well enough for those with determination enough to carry it out; the great difficulty, however, is that many have not the determination.

Morphinism. Dr. C. F. Barber. Quarterly Journal of Inebriety, April, 1889.

The author discusses briefly the effect of morphine, and states his belief in the gradual reduction treatment, together with some particulars as to his method of procedure.

Ueber die Geistesstörungen des Senium. Prof. Fürstner. Archiv f. Psychiatrie, Bd. XX, H. 2.

The basis of this study is furnished by 95 cases, all over 50 years of age, selected from a much larger number as distinctly senile. Hereditary predisposition could be traced in only 20 per cent, and rather as affecting the brain by way of the circulation than directly. The immediate occasion of the trouble may be change of long established habits of work, removal from familiar and congenial circumstances, bodily disease, failure of the special senses, inebriety developed late in life, etc. The following table classifies the cases by forms of alienation:

Form of alienation.	No. of cases.	Cures.	Betterments.	Deaths.
Melancholia simplex	33	11	7	3
Melancholia agitata	18	5	2	4
Melancholia stupida	3	..	..	..
Mania	9	3	2	..
Paranoia (*Verrücktheit*) in more or less abortive form	7	3	..	..
Delirium (*Verworrenheit*)	11	6	1	2
Dementia senilis	5	..	..	..
Dementia, with organic brain changes	9	..	..	..

For the full description of these as influenced by age, the original must be consulted. It may be said in general, however, that the melancholias are almost always tinged with hypochondria. Simple melancholia is less deep and shows a tendency to remission. On the side of the will there is great weakness, with abrupt conclusions and violent acts. Suicide is attempted on insignificant occasion and without the customary warning in intensified depression. The movements in *melancholia agitata* are more unceasing than in younger patients. Mania was never in these cases free of intellectual defects, which showed themselves in a less copious flux of ideas, and in greater carelessness of consequences in action. Illusions, especially of hearing, are frequent, and their growth from simple subjective noises can often be followed. Their elaboration is generally incomplete, and never, in Fürstner's experience, reaches paranoia; the excitement and tendency to violence are also less. An important and apparently little considered form is a delirium with hallucinations. Atheromatous processes are important in its causation; the patients are persons that have led active and exciting lives with many excesses; there are prodromal headaches and oppression, light attacks of dizziness, constipation, icterus, insomnia, a lachrymose mood, irritability, failure of memory. Unlike other senile insanities, the disease begins in an acute attack, with extreme excitement, or after a spell of unconsciousness, and goes on rapidly to complete confusion and disorientation. In the production of these, illusions and hallucinations play an important rôle. The mood varies, but is generally depressive or anxious. There is compulsion to movement both purposeful and erratic, but less than in the *hallucinatorische Verworrenheit*, which it resembles in this initial stage. There are marked disturbances of circulation, and often of digestion, and cerebral symptoms that sometimes suggest meningitis, but these are transient. After this first period, which may last for weeks or months, a part of the cases slowly recover, others continue much longer, though without the motor feature; the chances of recovery are greater than in *melancholia agitata*. Its connection with the circulatory system is further shown by the beneficent effects of digitalis. General paralysis Fürstner considers doubtful after 60 years, and he has not found the associated disease of the cord which more and more appears to be a feature of that disease. In senile dementia the moral ideas seem of little stability; intellectual symptoms are generally not found pure, but appear combined or alternating with melancholia and mania.

Insanity following Surgical Operations. C. T. Dent. Journal of Mental Science, April, 1889.

A surgical operation may disturb the mind by anticipation; by the pain, the relief or the shock of the actual operation; and by its after effects, the letting down of the mental tension, or the absence of a physical part that has been the center of over-subjectivity. It is to attacks of insanity which follow the operation, after a short period of normal mentality, that Dr. Dent devotes the bulk of his paper. He reports a number of cases, several from his own practice, but without attempting to trace the origin of the trouble on its psychic side. In a majority of the cases no tendency to insanity, personal or hereditary, could be found, and yet acute and chronic mania followed in some, and melancholia and dementia in others. The interval of sanity distinguishes such cases from those where the disturbances are due to the anaesthetic; and the trouble is not to be traced to a special antiseptic material, for the same was not always used. The author believes that these cases are rather overlooked than rare, and writes to encourage their observation.

Ueber die Auslösung von Schmerzempfindung durch Summation sich zeitlich folgender sensibler Erregungen. Ein Beitrag zur Physiologie des Schmerzes. Naunyn. Archiv f. experim. Pathol. u. Pharmakol., Bd. XXV, H. 3-4.

Somewhat in continuation of earlier studies on disturbances of sensibility in tabes, the author now reports experiments on a peculiar kind of hyperaesthesia in certain cases, mostly of tabes, in which the summation of stimuli, individually painless, produced pain. The stimuli, 60-600 a minute, were induction shocks or touches with a needle, blunt wire, or fine hair-pencil. Successful application was generally limited to areas on the sole and top of the foot, which were inconstant as to position, and sometimes wholly disappeared. The cases fall into two groups. In one the pain entered somewhat abruptly after from 3 to 45 secs., rose to a maximum, and after a few seconds ceased, whether the stimulation ceased or not; occasionally it returned after a period equal to the first delay. The pain was generally located at the point of stimulation, but sometimes extended over one leg or even both. It was accompanied also in many cases by reflex movements of the skin. Single rather severe needle-pricks produced the same kind of pain (in both cases disproportionate to the stimulus) after a delay of 2-4 secs. In the second group the pain did not cease while the stimulation continued, reached its maximum more slowly, and the reflexes were less marked. There was here less delay in the pain produced by single stimuli than before, and sometimes none at all. In a single case of transverse disease of the cord the pain was frequently felt on the other side at the point symmetrical to that stimulated. For the details of the experiments and the variations of result with individual subjects, as also for a full clinical description of the cases, the original must be consulted. The most important general result was that the length of the delay in the entrance of the summation pain depended far more on the rapidity than on the kind or intensity of the stimuli. The same was found by Stirling and by Ward for reflexes, and other points of similarity are traced by the author. Pain of the kind in question, and perhaps all pain, he holds, depends on summation, like the reflexes. Things that cause pain in common life are such as might well cause the nerve fibers to convey prolonged stimulation to the centers. The physiology of sensation may be conceived thus: Moderate stimuli are received by the end-organs, and the excitations are conveyed in the ordinary sensory tracts (probably the posterior columns of the cord) and do not summate. These tracts are, however, unfitted and inadequate for the excitations that result in pain (stimuli attacking the nerve-fiber itself directly or indirectly), and they are obliged to take others, probably in the gray matter of the cord, where they suffer summation and consequent delay, and cause pain. When the ordinary sensory tracts are useless from disease, moderate sensations are forced to take these other tracts, and so can also summate and cause pain, as in the cases experimented upon. If the gray matter itself were much affected, as in syringomyelia, this could not happen.

Ueber Wortneubildung bei Geisteskranken. Dr. BARTELS. 22. Vers. d. Vereins d. Irrenärzte Niedersachsens u. Westfalens. Allg. Zeitschr. f. Psychiatrie, Bd. XLV, H. 5–6.

An interesting detail in some cases of insanity, especially of long-standing paranoia, is the coining of new words. Besides simple misapplication of real words, there are some that are evidently made from words of similar sound, and still others to whose meaning there is no clue. There may be difficulty in finding out the signification of these from the patient, because he is offended at being asked to explain what he is sure are common and proper designations, or because of his suspiciousness. Four cases are given by Bartels, and in three there is abundant illustration of the new words used. One case did not know what some of the strange words meant, had perhaps heard them some time; another said they were revealed to her; a male patient, that they were given or brought to him, or arose through telephonic connections. From these explanations the author concludes that they originated in auditory hallucinations.

Hallucinations, and the Subjective Sensations of the Sane. D. HACK TUKE, M. D. Brain, Jan., 1889.

What is the seat of hallucinations? Is it peripheral, as Brewster thought? or central, as Esquirol believed? or is it the optic thalamus, as Ritti would have it? The author shows that none of these theories fits all the cases; there are hallucinations of peripheral origin, and as certainly of central origin, and, as against Ritti, of cortical origin. He discusses those of sight, and gives a number of interesting cases of his own observation, some sane and some insane, whose hallucinations he has been able to study more or less carefully. From a collation of these he arrives at some general means of distinguishing their seat, in substance as follows. Pressing the eye-ball to one side doubles only such things as are external to the eye, and so distinguishes real objects from hallucinations. Déspine, on the contrary, reports an insane patient with an hallucination of the Virgin that could be doubled. This the author does not try to explain, but leaves it as a counter case to those of his own observation. One of his cases, as one of Ball's and the subjects of hypnotic hallucinations secured by Binet and Féré, he regards as a case of illusion; and illusions, having a kernel of reality, behave like real objects. An after-image on the retina may obscure real objects, moves with the eye, and is projected. If these characteristics are found in an hallucination it involves the retina. Those that do not may occur when the patient is blind and the optic nerves atrophied. The interesting phenomena of the projection of dream images, unilateral hallucinations and those produced by drugs, are touched upon. From suitable experiments with the last, thinks Dr. Tuke, something may yet be learned in regard to their seats, though the results so far have been anything but definite.

Ueber subjective Gehörswahrnehmungen und deren Behandlung. Dr. EICHBAUM. Berlin u. Neuwied, 1888. Heuser's Verlag, pp. 32.

The subjective sounds are here treated from the standpoint of the aurist. Those that are really pathological may arise from disease anywhere in the auditory apparatus from the periphery to the cortical centers. Those of origin in the sound-transmitting apparatus are most common, and almost all of them come from too great pressure on the labyrinthic fluid, in an hyperaesthesic condition of the nerve. Those from disease of the outer ear (rare) and of the drum are not apt to be strong or continuous, but some of those from the middle ear (more frequent) become so. Others come from trouble in the labyrinth itself. These are generally loud and accompanied by the symptoms of Mènière's disease. Some arise from focal and general diseases of the brain or auditory nerve, and yet others from drugs and from general states of nervous disturbance, as hysteria and neurasthenia. The author also takes up prognosis and treatment. Here and there in the pamphlet are items of more direct psychological interest, for example, the remark that a man's business is apt to fix the character of the subjective sounds he hears (metal workers hearing hammering, musicians tones, etc.), and that the apparent intensity of the sounds may vary with the time of day and the mood of the patient.

In the *Annal. univers. di Medic. e di Chirurg.*, Vol. 285, April, 1888, Prof. Raggi describes two cases of unilateral hallucination. One was that of an alcoholic man, with delusions of persecution and bilateral hallucinations of sight. His unilateral hallucinations were of hearing and on the left side—noises, and voices defaming and accusing his wife. As these were only heard on that side, he concluded that she had tried to kill him by pouring poison into that ear. The continuous noises would point to peripheral excitation, but no disease could be found. The second case was that of a perfectly sane woman of 70 (earlier in life "nervous" and syphilitic), who for 15 years had had subjective sensations of sight. For about 5 years these were flashes and momentary red glimmers before the left eye. They then appeared before the right eye, at the same time gradually decreasing and finally disappearing from the left, where a cataract was forming. Two years later the cataract was operated upon and the lights returned. They likewise disappeared from the right eye on the formation of a cataract there. Later still the left eye was blinded by chorioiditis, without destroying the sensations, which on the contrary developed at last into hallucinations of landscapes, palaces, persons, animals, etc., all still confined to the left side. In this case a peripheral disturbance probably gave rise to central hallucinations. Such a peripheral origin seems common. Prof. Raggi has gathered 15 cases of unilateral hallucination from the literature; 9 were of hearing, 6 of sight; the left side was affected 11 times, the right 4, thus falling in with the view that makes the left half of the body predominantly sensory, the right predominantly motor.

IV.—MISCELLANEOUS.

The Double Brain. H. MAUDSLEY, M. D. Mind, April, 1889.

How the two hemispheres co-operate for the work of one mind is a question the answer to which must at present partake of speculation. Maudsley's answer, though something less than demonstrative, recommends itself at a number of points. In a discussion of the motor functions of the hemispheres he shows that, like the eyes, they have a large field of action in common, but also partial fields not in common. The same may be assumed of their sensory functions. Their relatively greater independence as centers of consciousness does not wholly destroy their unity of function. That rests upon the unity of feeling and action, and these in turn on the unity of the organic life of the single body. The brain is not a superadded regulator of the body, but part and parcel of it and its representative. The hemispheres act together, however, only when they have been trained to act together, as the eyes learn by experience to unite their double images. One hemisphere may, perhaps, control what has become automatic, but both probably co-operate for close attention and for the best apperception. Loss of the unifying power and improper action of the hemispheres makes mental disturbance. Mania and melancholia may be conceived as resulting respectively from an elevation and depression of the unifying power, the "disintegration of the ego" attending epileptic attacks from its perversion. For abundant illustration of the theory in the case of abdominal wounds, dreams, the powers of erratic geniuses, etc., the original should be consulted.

Muscular Movements in Man and their Evolution in the Infant . . . . together with inferences as to the properties of the nerve-centers and their modes of action in expressing thought. FRANCIS WARNER, M. D. Journal of Mental Science, April, 1889.

The emphasis laid by modern psychology on the motor side of mind makes such studies as those of Dr. Warner important. The first of the three sections of his article presents the relations of movements as to time, quantity, antecedents, delay, reinforcements, sequence, etc., gives something of the movements of different bodily parts, and shows the connection of movements with the nervous system. In illustration, fatigue and sleep are described in motor terms. The second section deals with the development of motion. At first there are certain reflexes and respiration. When the child is awake there are also more or less constant irregular movements, especially of the smaller members. These spontaneous movements the author calls microkinesis. They are not at first influenced by stimuli to sight and hearing, though the reflexes respond to touch. Reinforced action appears in the child's crying. In the following weeks the movements gain in force and extent, and new ones appear. At four months the child is affected by stimuli of sight and hearing; the microkinesis is temporarily inhibited by them (finely shown in graphic tracings), and a little later such inhibition is followed by general movements of response. There is evidence of retention, but no delay in the responses. At three years the control of the senses is very much widened; delayed and compound responses and those disproportionate to the stimuli are frequent; there is also imitation—all showing increased complication and interaction in the centers. At ten there is little microkinesis, and the responses to sensations are yet more delicate and varied. In the third section the author draws his conclusions as to the cerebral side of this evolution. The most important of these are connected with what he terms the diatactic action of the nerve cells, that is, their preparation for combined action. This diatactic action takes place in periods of inhibition, and is shown by the complicated motions which follow. Thought, which can be known only by motion of some kind, is the correlate of this diatactic action. The discharge of the functionally united cells need not, however, be directly into the muscles, but may spend itself in forming other unions. Motions of intelligence differ from others in their better adaptation. The cerebral qualities that favor such motions are given as follows: "1. Action in many small parts, not necessarily directly stimulated by any present or immediate antecedent forces. 2. Retentiveness and capacity for delayed expression upon a subsequent stimulation. 3. Capacity for the formation of functional unions among cells upon slight stimulation, such unions sending efferent currents to certain centers or muscles, with exactness, upon their stimulation."

Untersuchungen über den Musiksinn der Idioten. Dr. WILDERMUTH. Jahressitzung des Vereins der deutschen Irrenärzte, 1888. Allg. Zeitschr. f. Psychiatrie, Bd. XLV, H. 5-6.

The musical sense of idiots has attracted notice, but seems never before to have been specially investigated. The author has examined 180 of that class, and for comparison, 82 children from 7 to 13 years old. The less defective portion of the idiots were tested for compass of voice, certainty in giving a note, ability to distinguish the tones of a chord, musical memory, etc. They were marked on these tests by a credit system, and divided into four grades. The percentage of the whole number in each grade was as follows:

	Grade I.	Grade II.	Grade III.	Grade IV.
Idiots........	27 (16)	36 (29)	26 (36)	11 (19)
Children......	60	27	11	2

The percentages in parentheses are for a certain number selected from the full list whose mental state was that of children 2-4 years old, and who were in general considered incapable of education. Considering that the children had had a good deal of training and the idiots little, and that the investigation was made difficult by the general helplessness of the latter, and, in the case of many, by their lack of concentration, the showing is relatively as well as absolutely good. Female subjects, both normal and abnormal, were the more talented. In rhythm no errors were made, except by three idiots. With the still more defective portion of the idiots (30 cases) simpler tests had to be used (noises, a metronome, music-box, etc.), and the effect judged from aspect and gesture. Five failed to respond.

Most did not show displeasure at sounds unpleasant to the normal ear, while many did show antipathy to certain tones or noises not commonly unpleasant. The rhythm of the metronome pleased about one-third of them, and all found the music-box agreeable. The ways of expressing pleasure were various, but always exactly the same in a given individual. The emotion reached its height in 5–20 seconds; after 2–5 minutes it gave way to rather sudden fatigue. If the music continued, another accession of emotion followed, after an interval of indifference, but all reaction failed after 15–20 minutes of continuous playing. Five could hum tunes and learn to hum new ones. In five cases of acquired motor aphasia, the musical sense, both active and passive, was injured or destroyed, while in three of congenital origin it persisted; of twelve that were aphasic from intellectual defect, only two failed to respond. The response of such defectives, especially as compared with that to other aesthetic stimuli, testifies to the very fundamental nature of rhythm and music.

On Alternating Sounds. Dr. F. Boas. Amer. Anthropologist, Vol. II, p. 47, Jan. 1889.

"Alternating sounds" in language are such as may stand interchangeably the one for the other. A philologist in reducing a savage language to writing may at one time write *păc*, at another *bas* for the same word. These variations are due, as the author believes, not to real alternations of the sounds, but to alternations of apperception on the part of the hearer. In the same way he explains the mishearing of words attributed to "sound-blindness" (see experiments of Miss S. E. Wiltse, Amer. Jour. Psy. I, 702). The philologist on hearing a sound that falls between two familiar ones apperceives it first as one and then as the other, or he may hear sounds really different as one and the same. In the "sound-blindness" experiments the mishearing is not entirely at random; but the sensation of some letter-sound or word, varying slightly for some reason from the usual one, is heard as some other sound or word known to exist in the language

Un nouveau cas de guérison d'aveugle-né. Charles Dunan. Revue Philosophique, January, 1889.

A little girl, thirteen years old, was successfully operated upon for congenital cataract in the right eye, the left being hopelessly lost. Her previous seeing had been limited to distinguishing day and night. Two days after the operation the bandages were removed and a few tests made by the surgeon. Eight days later she was seen by Dunan and other tests made. Her perception of depth in space (monocular, of course) was very imperfect. She did not, however, perceive objects as in her eye or touching it, but saw them projected apparently at an indeterminate distance. Her perception of form, size, and direction was good. She said a disk of paper was round and white (she had seen some round objects and been taught the colors since the operation); she told which was the larger of two rectangles of paper; she reached in the right direction to grasp objects. The author goes to some trouble to prove that her condition was practically unchanged from the first, a thing which it is hardly necessary to say does not take the place of proper experiments at the removal of the bandages. The last part of the paper is given to a discussion of nativism and empiricism from the standpoint of the psychic synthesis theory.

Eine Beobachtung von Autohypnose. A. FOREL. Münchener med. Wochenschrift, No. 3, 1889.

Prof. Forel describes a not infrequent experience of his own, the original notes of which, in this instance, were made in 1878. He is sleeping in his easy chair against his will, his head on his right hand and his elbow on the arm of the chair. He struggles to wake up, succeeds in moving his left arm a little, and repeats the motion, but cannot increase it. Another effort lets him half-open his eyes, and he can see that his arm actually moves. He cannot move his body, but by and by is able to raise his head a little. It drops back, and his arm having moved a little forward, his eye is brought against the lower joint of his thumb. This is repeated seven or eight times, while he makes desperate and resultless efforts to get his hand under his head again. At last, however, he secures command of his body, and then of his legs, and takes pains to wake himself beyond peradventure. His sense of hearing is awake at such times, but liable, like the muscle-sense, to dream deceptions. The state is not a sleep condition of special muscle groups, but of special coordinated movements. Prof. Forel at present considers that at such a time he is auto-hypnotized, and that the auto-suggestion of motor inhibition grows in strength as his repeated and unavailing efforts convince him of its power.

Dreams, Sleep, and Consciousness. GEO. M. GOULD, M. D. Reprint from "The Open Court" of January 24 and 31, 1889.

This article is speculative, not to say fanciful. When the author announces his intention "to study the nature of consciousness and of its origin, from the facts of sleep and dreams," and presently takes up the question of "What is Consciousness?" the non-speculative psychologist may be excused for hesitation. The article, however, contains suggestive points, not the least valuable being the author's remarks upon his own dreams, etc.; for example, that in which he describes the experience of being consciously awake and yet struggling to rouse the somnolent motor centers—apparently the same as that described by Prof. Forel above. Cases of this kind are, perhaps, not extremely rare; a third has been personally reported to the writer of this note.

The Moods of the Sane. A posthumous paper by J. MILNER FOTHERGILL. Alienist and Neurologist, April, 1889.

The author takes up in a popular way, with entertaining anecdotes and citations from literature, the dependence of mind on body in normal and abnormal states of the latter; for example, the relations of courage and diet, the dullness of the anaemic brain, the characteristic moods of dyspepsia, consumption, cancer, etc. At no point is the influence of body more striking than in these all-pervasive changes of emotional tone.

Economy in Intellectual Work. WM. H. BURNHAM. Scribner's Magazine, March, 1889.

This is an attempt to outline the principles that underlie economic mental activity. The relation of unconscious cerebral processes to the problem of economy is especially considered. Adopting the theory that bases attention upon emotion, the writer urges that an emotional stimulus is necessary for economical mental work, and concludes that emotional dissipation in childhood and at adolescence is likely to leave the mind barren of healthy interests and without emotional support in its intellectual activity.

Sonnenschein's Cyclopaedia of Education. Edited by ALFRED EWEN FLETCHER. Syracuse: C. W. Bardeen. London: Swan Sonnenschein & Co. 1889.

Among the contributors to this convenient hand-book are Oscar Browning, Sir Philip Magnus, James Sully and other eminent educators. It covers a wide field, containing not only articles relating to the history, theory, and practice of education, biographical sketches, explanations of pedagogical technicalities, and the like, but also articles upon the mental activities, the sentiments, virtues, and other psychological and ethical topics. In the psychological articles the attempt is made to show the pedagogical aspect of the subjects treated. Although its psychological horizon is necessarily limited, the book is a valuable addition to educational literature. An excellent bibliography of thirty-four octavo pages is appended.

W. H. B.

Memory as a Power of Knowledge. WM. L. EVANS, M. A. New York, 1888.

This book outlines the psychology of memory and the association of ideas, and contains a mnemonic system based on the principles of the Pick-"Loisette" method. The author adopts a figure alphabet, connects dissimilar words by "intermediates," and gives mnemonic series of words for practice. One of the best features of his psycho-pedagogical discussion is the emphasis placed upon the training of the attention. It may, however, be doubted if the author's method of effecting this training —*i. e.* by the treadmill recitation of mnemonic series—is altogether the best one.

Report of the Committee on Phantasms and Presentiments. JOSIAH ROYCE. Proc. Amer. Soc. for Psychical Research, Vol. I, No. 4, March, 1889.

This report is the most important and most extensive of those in this number of the *Proceedings*. The material upon which it is based consists, so far as published, of between 70 and 80 cases, of which 33 are considered in the body of the article. These are classified and treated under the following headings: I. Subjective Hallucinations of Familiar Type; II. Instances of Recognized Sorts of Unconscious Cerebration; III. Pseudo-Presentiments, and, IV. Coincidences. The most important feature of the report is the theory of Pseudo-Presentiments, advanced in the third section. In a word, it is that normal persons are occasionally subject to a trick of memory something like that which gives one in a new place the feeling, "I have been here before," but which in this case takes the form, "I have already dreamed this, or had a presentiment of it, or it has been prophesied to me." Outside of dreams, where the experience is not infrequent with some people, it is difficult to get perfectly clear cases, for it can rarely be proved absolutely that the person reporting such a coincidence has not happened to have such a dream or presentiment before the event in question. The explanation is, however, extremely plausible, has indirect evidence, and a very extensive explaining power. Under the fourth heading Prof. Royce gives a few cases in which a thought, dream, or presentiment of one person coincided more or less closely with an experience of some other person. Of these cases, 3 are supported by documentary evidence, 4 by strong testimony without documents, and 5 are of less certainty.

NOTES.

Exner and Paneth (*Pflüger's Archiv*, XL, p. 544), repeating the experiments of Marique, found that when those parts of the brain of the dog which contain the motor cortical fields for the extremities are cut around, so as to sever their association-fibers but not their projection-fibers, care being taken to injure as little as possible the blood-vessels of the pia mater, the dog showed all the symptoms which follow complete extirpation of the same part. The authors attribute the atrophy which sets in, in part to disturbances of nutrition, and in part to the separation of the association-fibers. As in the case of extirpation, nearly complete recovery of function takes place after a few weeks or months.

The paralytic brains among those of 453 East-Prussian insane studied by Dr. Julius Jensen (*Archiv f. Psychiatrie*, Bd. XX, H. 1), showed a deficiency in weight of about 20 grams for each year of disease. The atrophy, as indicated by the weight of the separate parts (divided according to Meynert's method), seems to spread over the mantle from in front; the axial portions are also much affected. In melancholia the frontal portions are not affected, though the mantle as a whole is light. The normal proportion of the mantle in 1000 parts is 785.82, in melancholiac men 780.01, in melancholiac women 779.31. Taken altogether the figures show the right half of the brain heavier than the left.

Dr. Tigges has studied in the same way the brain weights of 123 insane men and 127 insane women in Sachsenberg (*Zeitschr. f. Psychiatrie*, Bd. XLV, H. 1-2). The average weight with membranes was respectively 1362.3 and 1243.6 grams, the variation of the individuals from the mean being greater with the insane than the sane. The averages for different forms of alienation for the men were: mania, 1430.7; melancholia, 1392.8; primary forms in general, 1402.3; secondary forms in general, 1401.3; simple psychoses, 1401.7; paralysis, 1283.7; epilepsy, 1362.3; in the last there was considerable variation. For the women the weight after melancholia was higher than after mania, and the difference between primary and secondary forms greater. The brains of female epileptics were relatively heavier than those of the males, and of female paralytics, lighter. Rather heavy brains were found after periodic insanity; men, 1400.3; women, 1347.5. Taken separately, the brain mantle was a little heavier in proportion to the whole in the men; the cerebellum and axial portion a little heavier in the women. The mantle is relatively light in the insane, the remainder of the brain heavy. Most of the male brains were heavy in the tempero-occipital and parietal regions, the female in the frontal, but there were not a few exceptions. Tigges, like Jensen, found the frontal portion heavy after melancholia, very light after paralysis; heavy also in delusional insanity and light in mania—suggestive facts, in his opinion, in view of some features of these troubles. In epilepsy the proportions are irregular. The right hemisphere in the insane was found, as above, heavier than the left, and more so than in the sane; the frontal portion was always heavier on the right, and the parietal and tempero-occipital on the left, except in primary psychoses. The difference of the hemispheres was greatest in idiocy, epilepsy and paralysis, where also the left was frequently the heavier. The period of greatest weight for the male was from 30 to 80 years, for the female 40 to 70. The weight of the brain increased with the length of the body, faster in women than men, though the weight as compared with the length was greater in men. From a comparison of his results with those of others Tigges is inclined to believe in different average weights for the different German populations.

After two years' experiments as to the optical disturbances following lesions of the cortex in monkeys and dogs, Lannegrace has arrived at a set of results quite at variance with those now accepted (vide *Archives de méd. expériment. et d'anat. pathologique*, Jan., 1889). Instead of finding such disturbances only after injury to the occipital region and adjacent parts, he finds hemiopia after injuries in almost any part of the cortex (the occipital region being an important but not an exclusive center), and crossed amblyopia after injuries to a limited area in the parietal and frontal regions. Lannegrace himself notes the difficulties of determining the exact nature of the visual effects produced and the frequency with which the experimenter is obliged to depend on general impressions. In these, perhaps, lies the secret of the differences.

H. J. Hamburger (*Feestbundel van Donders*, 1888) experimented on the time necessary to produce a perceptible change in the color of the visual purple in the eyes of frogs. For the different parts of the spectrum, the time required was, in hours,

C	D	E to b	b⅓ to F	F
40	20⅓	7½	10⅓	14

And when the width of the slit was made inversely proportional to these times, then lights of all wave-lengths produced a just perceptible change in seven and one-half hours.

When one looks with the head inclined to one side at a bright vertical line in the dark, the line appears inclined to the opposite side. This phenomenon, discovered by Aubert, has been explained by him and by Helmholtz as due to an under-estimation of the deviation of the head, the rolling of the eyes under such circumstances being insufficient to account for it. M. E. Mulder, who has recently given attention to the subject (*Feestbundel van Donders*, 1888, p. 340), though in doubt as to the true explanation, thinks this one at least insufficient. He finds that even when the head is known to be horizontal, the illusion amounts to 20° or 30°. It does not vary in accord with the errors made in estimating the position of the body when inclined to one side or the other in a movable box. If the eyes are closed for an instant and then opened, it is at first less, but presently regains its former amount. It varies with the individual from 10° to 60°, and with the inclination of the head. It persists in spite of practice.

In an inaugural dissertation, *Ueber die Messung der Tonstärke* (Berlin, 1888), M. Wien has described a new phonometer and a series of interesting experiments executed with it. Applied to the testing of Weber's law, the apparatus gave with a tone of 440 vibrations the following values for the discriminative sensibility:

Intensity.	Discrim. Sens.	Intensity.	Discrim. Sens.
Circa 1.6	(Threshold)	Circa 10^6	.140
5	.135	10^7	.153
20	.108	10^8	.161
10^2	.112	10^9	.178
10^3	.118	10^{10}	.225
10^4	.116	10^{11}	.350
10^5	.131		

The discriminative sensibility is thus finer for tone than for noise (that usually measured); it is finest for an intensity about ten times the threshold value; and gradually becomes blunter as the intensity increases. Weber's law holds approximately for a part of the series. The power of discrimination was found to be strongly affected by the pitch of the tone. The absolute changes of pressure at the threshold are given at 0.59 $\mu\mu$ of mercury, and the amplitude of the vibrations of the air particles at 0.066 $\mu\mu$ ($\mu\mu$ being one millionth of a mm.), a value about one-seventh that given by Lord Rayleigh. The energy at the threshold is estimated at about six times that for sight (e. g. seeing a star of the sixth or seventh magnitude). The intensity of the first over-tone in the vowels, when the fundamental was represented by from 15 to 20, was about as follows: a 7, e 11, i 12.5, o 25, u 3.5, ü 5, and ä 10. In large open spaces the intensity of a tone decreased in close approximation to the increase of the square of the distance.

The following explanation of the function of the arches of Corti is given by Dr. C. Brückner in *Virchow's Archiv*, Bd. CXIV, H. 2. The physicists have shown that the tone of a vibrating rod, fixed at the middle, descends as the ends are bent around into the shape of a tuning-fork. Brückner himself got a somewhat similar result from rods fixed at the ends. Now the Corti arches at the base of the cochlea are small and little spread, those at the upper end large and much spread. The size and shape therefore work at cross-purposes, approximately compensate each other, and bring it about that all the arches can vibrate to each of the strings of the basilar membrane, like the sounding-board to the different strings of a piano.

In the same paper the author records an observation in support of the separateness of the organs for hearing noises and tones, which, though it is far from decisive, has been frequently made by him, and is not without interest. On waking during the striking of a clock, he hears first a continuous musical tone, and only at the last the noise of the single hammer-strokes—in his opinion, because the tonal organ awakes first.

In a recent article on Ataxy and Muscle-sense (*Zeitschr. f. klin. Medicin*, XV, 1-2; cf. abstract of an earlier communication, this JOURNAL, Vol. I, p. 324), Goldscheider has undertaken an investigation of the senses that mediate muscular co-ordination. He treats the subject under these heads: (1) feeling of passive and (2) active movements, (3) the perception of position, and (4) the sensation of weight and resistance. (1) A passive bending of the finger (at the second joint from the end) is perceived when it has reached 0.60-1.74°, and the perception is due, not to the slight sensation of pressure caused in moving the finger, but to sensation in the region of the joint; for, when the first is in a measure destroyed by faradizing the last phalanx, the perception of motion is practically the same, but when the joint is faradized the perception is much less acute. Indeed, when the electric stimulation is not applied, careful observation can distinguish one sensation from the other. That the sensation is chiefly in the joint and not in the skin about it is shown by the fact that when the skin was made insensible by faradizing, a motion of 3.14° could yet be perceived. The rapidity of motion is also of influence. (2) The results are the same in general for active motion, sensations in the tendons (which are here more important than in passive motions) perhaps accounting for the somewhat finer perceptions. The author is inclined to exclude muscle-sensations proper, because on tetanizing a muscle they do not appear till the contraction is much greater than the amount in question. The beginning of a motion does not require muscle-sensations, and when it is once begun the various sensations accompanying motion make it unnecessary. He would not, however, deny their existence in certain muscles of the eye and others not producing motion in a joint. He found also that while an idea of motion could be voluntarily called up without actual motion, the idea of the sensation of motion could not be. (3) The sense of position comes from the tendons and their connections and from the skin; and the author does not believe that the sensation of motion is derived from that of changing position, but considers it an independent and primary sensation. (4) He holds the sensation of weight as also peripheral. These sensations give the data for muscular co-ordination; ataxy is a result of their more or less complete absence. Bendings and straightenings of the faradized finger intended to be of a certain extent and rate were found to be irregular, too great, and too rapid. A greater motion is necessary under such circumstances to give a sensation equal to that experienced in the normal finger. The irregularities, the extent and the rate were increased by attention to the motion, but decreased when it was followed with the eye. The ataxic gait the author attributes to a decline of the sensations of movement in the knee and hip joints and of the sensations of tension in the tendons. This hinders co-ordination of antagonistic muscles; and with it probably co-operates the lowering of muscle tonus.

The distinct forms of periodic insanity so far recognized Professor Mendel would gather under the three heads of mania periodica, melancholia periodica, and delirium hallucinatorium periodicum. To these he would add paranoia periodica, and describes three cases in which the typical hallucinations and delusions of persecution and grandeur were present in periodic attacks. There was no delirium; one of the cases, a merchant, was able to attend to his business. A fourth case is also added, that of a man who, after nine periodic attacks of melancholia, had one of mania with initial melancholia, then one of melancholia with illusions and delusions, and finally a twelfth of full paranoia. (*Allg. Ztschr. f. Psychiatrie*, 1888, Bd. XLIV, H. 6.)

An interesting case of *folie à deux* is reported by Dr. M. J. Nolan in the *Journal of Mental Science* for April, 1889. The patients were brothers, both weak-minded, the younger infected by the older, he by his mother, and she by her husband. The two sons had lived together in almost complete seclusion, taking care of their mother, bed-ridden for seven years. The resulting similarity of mind was very great. After three months of separation at the asylum, they simultaneously announced the same delusion, agreeing even in details, and on another occasion dreamed the same night at nearly the same time of seeing their mother. The author takes the case as evidence that the identical ideas in this disease are not always communicated.

In the *Journal of Mental Science* for April, 1889, John Baker, M. B., discusses the incendiarism of the insane, chiefly on the basis of cases from the Broadmoor Asylum. From 1864 to 1886, 95 men and 8 women, respectively 7.5 and 2 per cent of the total commitments, were received for arson. Continental experience gives a larger proportion of women, many being servant-girls between 12 and 18 years old. The distribution among the forms of insanity was as follows: congenital imbecility 36, melancholia 21, congenital epilepsy 4, general paralysis 6, acute mania (usually *à potu*) 6, recurrent mania 4, chronic mania 7, monomania 9, dementia 10. While admitting a connection between insanity and a propensity for fire-setting, the author concurs in the generally accepted opinion against an instinctive "pyromania," and in favor of a more frequent connection with the reasoning forms of insanity.

In the brain and cord of dogs after acute intoxication with ethyl or amyl alcohol, W. Tschysch finds no histological changes; but regularly after chronic intoxication, puntiform hemorrhages, especially in the gray matter of the cord, together with exudation of plasma, degeneration and destruction of the nerve-cells, chiefly in the neighborhood of the vessels. The kind of lesion is the same with either alcohol, but the amylic is fatal in smaller doses. (Report of Proceedings of III Congress of Russian Physicians, 1889, *Neurolog. Centralbl.*, No. 7, 1889, p. 209.)

A case of etheromania is reported by Ritti in the *Annales médico-psychologiques*, Jan., 1888. The victim was a woman of about 40, well educated, but of unfortunate heredity. At 22 ether had been prescribed for gastralgia and weakness, and she had become dependent upon it, but had been able to break off. When prescribed at this later period for similar troubles attending metrorrhagia, she was not able to break off and came to use more than 200 g. a day. As her means failed she was reduced to street-begging to procure the intoxicant, and was found in public places stupefied from inhaling it. She was arrested and pronounced insane. Under compulsory abstinence she recovered in about six months, with one relapse. The effect of the ether was like morphine in giving the woman a liveliness and talkativeness quite different from her ordinary taciturnity, also in its abstinence phenomena, and the imperious nature of the craving it created.

Alcoholic paralysis has generally been considered a disease of the peripheral nerves, though degenerations have occasionally been found in the cord. Dr. Karl Schaffer observed in the cord of a female drunkard (*Neurolog. Centralbl.*, No. 6, 1889), who in life had been paralysed in the lower limbs, atrophy or sclerosis of the greater part of the cells in the anterior horns in the lumbar region, amyloid concretions diffusely scattered through the whole cord (thickest in the posterior columns), and apparent atrophy of single cells in the columns of Clarke. The anterior and posterior roots were normal, as were also the greater portion of the cells in the cervical region. The peripheral nerves could not be examined. Of nearly the same tenor were the findings in a case reported by Dr. A. Erlitzki to the Congress of Russian Physicians (reported in the *Neurolog. Centralbl.*, No. 7, p. 210). The very evident central effects of alcohol (drunkenness and psychic disturbances), with the degenerations in this and a similar case of Kahler and Pick's, lead Erlitzki to the opinion that the beginnings of alcoholic paralysis are in the cord.

To the happily small list of cases of delirium tremens in childhood, Dr. E. Cohn adds another case (*Berl. klin. Woch.*, No. 52, 1888). The patient was the five-year-old son of a saloon-keeper, who had been given, with all good intention, a glass of "Luft" (cummin brandy?) every day by his grandfather, some Hungarian wine by his mother, and had gotten besides more or less beer and sometimes an extra glass of "Luft." He was run over and brought to the hospital with a broken leg. The next day the delirium appeared accompanied by pronounced tremor, but with a dose of chloral hydrate was over in 24 hours. The leg healed well, the child in the meantime having had the measles. The close sequence of the delirium upon the injury and the withdrawal of alcohol, its short duration, and the tremor connected with it, are worthy of notice.

In a case of alexia of Dr. Brandenburg's (*v. Graefe's Archiv f. Ophthal.*, 1888, XXXIII, 3), the patient, while unable to read words, was able to read Arabic numerals. Dr. Brandenburg explains this exception somewhat as follows. When an uneducated man, like the patient, reads, it is necessary that the center of visual images, the center of auditory images of the letters (since he spells in reading like a child), and the center of speech-movement images should all be put in action, before the word that is read becomes an idea. With an educated man, on the contrary, the process is short-circuited and the word becomes an idea at once. This patient was in the latter state as regards numerals; though the roundabout way was broken, the other direct way was still intact. There is, however, some reason to conjecture a distinct location for number images; at least Oppenheim mentions a case in which disease of the right hemisphere was accompanied by loss of them.

In the *Deutsches Archiv f. klin. Med.*, XLIII (1888), 4–5, Dr. A. Knoblauch reports the case of a girl six years old, suffering apparently from encephalitis, with right hemiplegia and aphasia, who, at a time when she was quite unable to speak voluntarily or at dictation, retained the power to sing the words of a familiar tune. Even if such speech is automatic and may be mediated by the right hemisphere, the mechanism of tracts and centers by which this is done is still to be discussed. Into this theoretical question the author goes with fullness, deducing from his schema the disturbances of the musical faculty, and naming them after the analogy of those of speech, *e. g.* "amusia," corresponding to motor aphasia, "paramusia" to paraphasia, "tone-deafness," "note-blindness," etc. Some of them he is able to parallel with symptoms from cases on record.

For the cerebral weakness corresponding to neurasthenia, Prof. Finkelnburg suggests the term *phrenasthenia* (Jahressitzung des Vereins der deutschen Irrenärzte: *Allg. Zeitschr. f. Psychiatrie*, Bd. XLV, H. 5–6). The characteristic of phrenasthenia, as of neurasthenia, is the ready exhaustibility of the patient, though this is obscured in the first by the fact that it may affect either the active functions or those of inhibition, and give rise to a torpid or an erethic form. In the discussion that followed, Professor Mendel confessed himself a heretic on the doctrine of neurasthenia, believing the name to cover a group of functional neuroses. In Finkelnburg's analysis of phrenasthenia he saw a return movement toward the scientific standpoint. As evidence of the near connection of neurasthenias with actual insanity, Dr. Knecht mentioned a case in which torpid asthenia, erethic asthenia, and good health alternated like the stages of depression, excitement, and the lucid interval in circular insanity.

Evidence accumulates that men, even those in virile occupations —blacksmiths and soldiers—may be subject to hysteria. At the Congress of Russian Physicians at St. Petersburg, January 3–10, 1889 (*Neurolog. Centralbl.*, No. 7, 1889, p. 209), Dr. Oseretzkowski made further reports of his studies in the Moscow Military Hospital. He has now records of 38 cases (33 privates, 5 officers) in which the disease appears in its protean forms of paralyses, contractures, convulsions, mutism, etc. The doctor deprecates the tendency to regard hysteria in soldiers as simulation. The case of a merchant thus affected was also reported in the session of February 23, 1889, of the Association of Physicians of Budapest (*ibid.* p. 216).

In the January number of *Brain*, Dr. Wiglesworth and Thos. H. Bickerton report further investigation of the relation of epilepsy to errors of ocular refraction. In 103 unselected insane and imbecile cases they found 48 in which there was noticeable error. The percentage among the imbecile was a little less than among the other insane, indicating, as far as a small number of cases may, that these anomalies are not to be charged to general degeneration. These cases were not suited to the further step necessary to prove a causal dependency of epilepsy on errors of refraction, *i. e.* the cessation of the fits after correction of the errors, but in nine cases reported from private practice, all showing errors; some success was met. The authors do not, of course, regard the strain coming from these errors as more than an exciting cause to otherwise unstable nervous systems.

In spite of much investigation, the function of the thyroid gland and the manner in which the disturbances that often follow its removal are caused remain as yet uncertain. Th. Drobnick (*Archiv f. exp. Patholog.* XXV, p. 136), after operating upon 8 dogs, 3 of which survived, concludes that the gland is not essential to life, and that the disturbances of circulation and respiration, the convulsions, pareses, etc., that follow its extirpation, depend upon the excitation, directly or reflexly, of the neighboring nerves. The troubles are rather to be compared to tetanus than *cachexia strumipriva*. H. Schwartz, who has studied the relation of the sequelae to tetanus (*Inaug. dis.*, Dorpat, 1888) believes that they may be related in causation, but are not identical. Munk explains their genesis, in reporting further studies on the subject (*Sitzber. d. kgl. preuss. Akad. d. Wiss.* 1888, XL, p. 1059), as due to injury of the nerves lying near the gland; this causes disturbances of circulation and respiration, which in turn bring about mal-nutrition of the central nervous system, whence the convulsions, etc. To this points the fact among others that similar disturbances can be produced by injecting dilute croton oil into the region of the gland. To another experimenter (Rogowitch: *Archives de Physiol. norm. et patholog.* (4), II, p. 419), the results seem like those of a nerve poison, say of phosphorus. This poison, as he thinks, arises in normal processes and is counteracted by the gland. In this function the pituitary body can to a certain extent act vicariously. Clinical opinion as to the dependence of myxoedema on loss of the gland seems almost equally at variance (see a number of articles abstracted in the *Neurolog. Centralbl.* No. 5, 1889).

Dr. P. Grützner argues in the *Deutsch. med. Wochenschr.* 1889, No. 1, that the experiments of Munk and others, who have found no serious disturbances after removal of the thyroid gland, are inconclusive because there are frequently many little accessory glands widely scattered through the adjacent parts. In some fatal cases these have been found; in some cases of recovery also they have been present and enlarged. He believes on the whole that the disturbances cannot be explained by simple injury of the nerves, but that in addition a special toxic substance is developed, either in the wound or elsewhere in the body; and if the latter, the causal connection with the removal of the gland may be assumed.

In the February number of *Education*, Dr. C. F. Crehore gives a brief sketch of a system of anthropological measurements for use in schools. It includes both physical and mental measurements, and would, no doubt, lead to the collection of valuable information, though it might, perhaps, be improved. An adoption of the scheme used by Galton, or an addition of the points in which his differs, would give opportunity for most desirable combinations of data. The political economists have frequently found international comparisons impossible from such differences in the schemes by which their statistics are gathered.

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FOLK-LORE OF THE BAHAMA NEGROES.

BY CHARLES L. EDWARDS.

The Bahamas include over 3,000 islands, but most of these are small, grouped about a few larger islands and these groups are not separated by great distances, so that as a whole the Bahamas present a striking homogeneity, both in their origin as coral formations, and in the life and surroundings of the people. As the biologist of to-day gains a better insight into the complex problems of structure and relation presented by living things, by mastering the life history of one or a few species, so, in the study of the folk-lore of a people, can we gain a better knowledge of its philosophy by considering the physical environment and the history of some particular community of that people.

The material for this paper I gathered during the summer of 1888 at Green Turtle Cay, one of the "out islands" of the Bahamas, which because of its isolation presents the simplest conditions in the life of the people and has all of the physical features peculiar to the Bahamas. One of the broken parts of an ancient fringing reef to the island of Great Abaco, of only a few square miles in area, this island, as its name (from the Spanish *cayo*) indicates, is only *a rock* in the ocean. Like a hundred others in the same series it appears from a distance as a dark low-lying sand-bar, but when closely approached is seen to be covered, for the most part, with short trees and bushes, while occasionally a clump of tall cocoa-palms is sharply outlined against the bright sky. The vegetation is of as deep a green as the sea is blue beyond the present coral reef, while between the vegetation and the water the triturated skeletons of corals and echinoderms and the shells of molluscs, constituting an intensely white coralline sand, glare in the sub-tropical sun. The shoals about are also of this constantly shifting sand, and so the shallow water is rendered a chalky green shade, beautifully contrasting with the wonderful blue of the deeper sea.[1]

The island of Great Abaco, or in native vernacular, "the main," lies about three miles away to the South-west, while to the North and East, unbroken but by the top of the present coral reef, marked at each wave by a white line of spray, extends the vast Atlantic.

It is on the south-west side where the wide channel between the Cay and "the main" affords such an excellent roadstead for vessels, that the town has been built. In this channel, the swell of the ocean, so much broken by the outer reef and the line of cays, is perceptible only in the small passages between cay and cay, and to a less extent along the shore of Great Abaco, while for the most part this water is as smooth as a lake, disturbed only by the tides and winds. It is a sight of peculiar beauty to see on a summer morning the small boats of the "Conchs," as the natives both white and black are called, scudding over the blue waters of the channel to points on "the main" for ten miles on either hand. They go to the "pine fields;" for the raising of pine-apples is the principal farming industry at Green Turtle Cay, and most of the men who cultivate the plants live in the town on the cay, sailing to their farms in the morning and back at night.

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[1] These shoals sometimes reach gigantic proportions, as in the Great Bahama Banks where over hundreds of square miles the water is but a few fathoms in depth.

There is practically no soil as nature leaves the land, and so the plants found there are such as can adapt themselves at the root to crannies in the rock and there gain some sustenance from the mould of their ancestors, while from the air, the leaves may breathe in the rich supplies of gasses and moisture, always emanating under the sub-tropical sun. So farming implements in the Bahamas, as has been aptly and almost truthfully said, are the pick-ax and *machete.* With the former a natural crevice in the rock is somewhat widened, and therein a pine-slip or some seed is planted, while with the *machete,* a long, broad-sword sort of knife, the weeds and bushes are cut down.

The town of Green Turtle Cay was founded by a family of loyalist refugees who fled from the American colonies during the revolution. It has grown slowly, for the most part by the natural increase of the few first families; and, because of repeated intermarrying among these family stocks, at present nearly all of the people are inter-related. The population, about evenly divided on the basis of color, is of some fifteen hundred souls.

If one will imagine a sea-coast town in North Carolina, (as much as possible isolated from railroads and ocean steamers, and its people leading a seafaring life with farm work at odd intervals,) transported to a small coral island, then one can gain a very fair outline for the picture of Green Turtle Cay. But there are touches of local coloring quite necessary to complete the picture.

There being no horses and carriages on the cay, roads for their accommodation are not essential, and so the streets are not wider than a city sidewalk, and the squares, into which the town is divided, are proportional to the streets in size. The streets are formed by smoothing off the naturally jagged points left by the action of water upon the coral sandstone of which the cay is composed, and they are of dazzling whiteness. The houses are generally of frame, three or four sometimes crowded upon the same small lot, and, whenever the owner can afford the display, painted white, a most disagreeable reflection of the glare from the street and sea-shore.

The people are intensely pious. The whole social life centers in the church. Those mad days of the Buccaneers and their nominally more respectable descendants, the Wreckers, are gone. For the ribald song of the riotous pirates, we have the solemn hymns of the Wesleyans, and the chant of the English church. Light-houses have taken from the coral reefs their former terror. But after some residence among the people, one is compelled to suspect a shallowness in their piety, a great deal of selfishness in their character. Their conceit and petty social distinctions are such as all isolated provincials exhibit.

The laws against swearing are quite severe and, what is even more necessary, the good old patriarch who holds all of the offices from chief magistrate to street commissioner, is quite strict in the enforcement of the laws, so that the ordinary street talk is quite a relief to one who is familiar with the profanity of American streets.

The colored people, everywhere gossipy, good-natured and religious, having here been emancipated for over fifty years, have become somewhat educated and unusually independent. Socially the races are more nearly equal than anywhere else on the globe.

Schools and churches are occupied in common. Miscegenation, so prevalent in Nassau, the capital, has not prevailed in this colony to any extent. The first few negroes who came to the cay were slaves of the loyalists, and then from a shipwreck about 1837 a few more from America were stranded in this colony, but aside from these, the large majority indeed, have come as direct descent from native Africans, and there yet lives one old negro "Unc' Yawk," who, bowing his grizzled head will tell you: "Yah, I wa' fum Haf'ca."

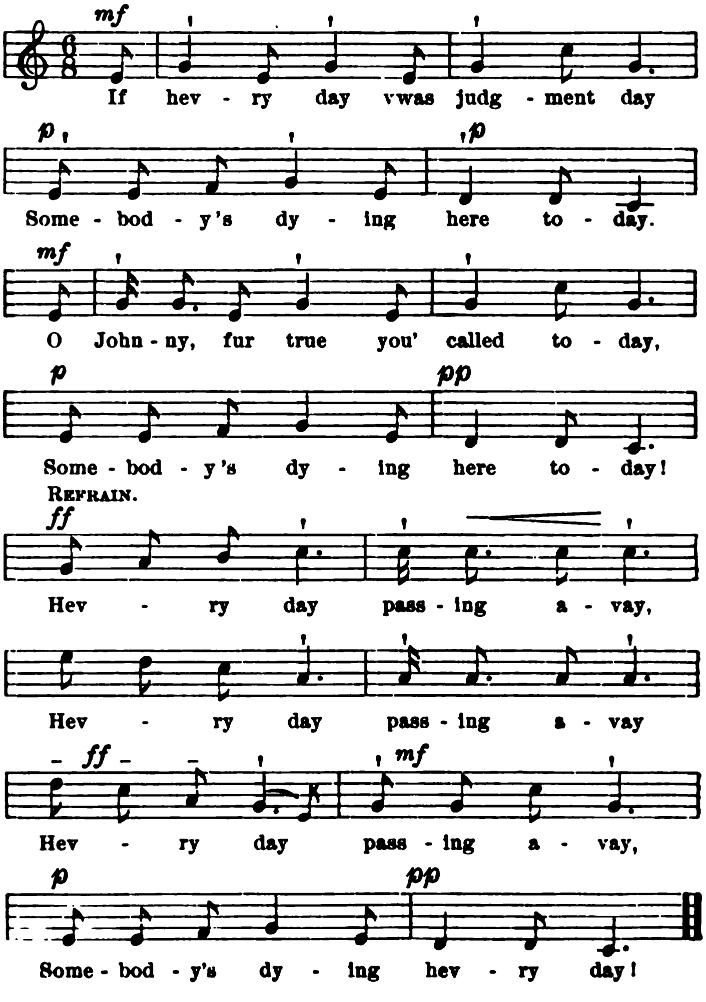
It is with the negroes that one associates the picturesque and beautiful surroundings in the Bahamas. Their huts, so often thatched with palmettos, are built on the low, sandy soil of the town. There grow the graceful cocoa-palms with long green leaves whose leaflets rustle as sadly as do those of oak and chestnut in the autumn woods of the north. There, too, the prickly pear, like an *abatis*, bristling all over with needles, seems to guard the luxuriant blossoms of the great oleander bush, dispensing sweetest perfumes from its midst. Apparantly every hut has its quota of a dozen little black "Conchs" of assorted sizes, who think it a palace beneath the palmetto roof and the yard a menagerie wherein the pigs, and chickens, and dogs are animals worthy of study.

There are no chimneys in the Bahamas and but few stoves. Boiling and frying are done in a small shed over an open fire, built on a box of sand; while for the baking is employed an oven of the same sort as our foremothers knew by the name of the "brick oven." It is a cone made of coral sand-stone into the upper half of which is hollowed an oven. In this oven a fire is built and kept burning for several hours until the rock is quite hot, then the fire is raked out and the food to be baked is placed in the oven. The "mammy" and children do most of the work while the lord and master plays checkers or lies in a hammock reading a novel.

There is one piece of work however in which man and wife share, and that is the chastisement of the children. They chastise with a club, and regularly every twenty-four hours the screaming of the tortured child comes from the hut or surrounding bushes, to tell its sad tale of remaining barbarism; but the negro child has a disposition full of sunshine, and in a few moments after being beaten, will sing like the happiest being on earth.

In the evening is the play time of the negroes. The children gather in some clump of bushes or on the sea-shore and sing their songs, the young men form a group for a dance in some hut and the old people gossip. The dance is full of uncultured grace; and to the barbaric music of a clarionet, accompanied by tambourines and triangles, some expert dancer "steps off" his specialty in a challenging way, while various individuals in the crowd keep time with beating of feet upon the rough floor, and slapping of hands against their legs. All applaud as the dancer finishes; but before he fairly reaches a place in the circle, a rival catches step to the music and all eyes are again turned toward the centre of attraction. Thus goes the dance into the night.

The strangest of all their customs is the service of song held on the night when some friend is supposed to be dying. If the patient does not die they come again the next night and between the disease and the hymns, the poor negro is pretty sure to succumb. The singers, men, women and children of all ages, sit about on the floor of the larger room of the hut and stand outside at the doors and windows, while the invalid lies upon the floor (which Bahamans generally, both white and black, prefer to the bed) in the smaller room. Long into the night they sing their most mournful hymns and dirges and only in the light of dawn do those who are left as chief mourners silently disperse. The following dirge is the most often repeated, and with all their sad intonation accented by tense emotion, it sounds in the distance as though it might well be the death triumph of some old African chief:


mf
If hev - ry day vwas judg - ment day
p
p
Some - bod - y's dy - ing here to - day.
mf
O John - ny, fur true you' called to - day,
p
pp
Some - bod - y's dy - ing here to - day!
REFRAIN.
ff
Hev - ry day pass - ing a - vay,
Hev - ry day pass - ing a - vay
ff
mf
Hev - ry day pass - ing a - vay,
p
pp
Some - bod - y's dy - ing hev - ry day!

Each one of the dusky group, as if by intuition, takes some part in harmony, and the blending of all pitches in the soprano, tenor, alto and base makes such peculiarly touching music as I have never heard elsewhere. As this song of consolation accompanies the sighs of the dying one, it seems to be taken up by the mournful rustle of the palms and lost only in the undertone of murmur from the distant coral reef. It is all weird and intensely sad.

The folk-tales are most popular among the children, and, indeed, are no doubt handed down from generation to generation principally by them. After the short twilight and the earlier part of the evening, when singing and dancing amuse the children, comes the story-telling time *par excellence.* This is usually about bed-time and the little "Conchs" lie about upon the hard floor of the small hut and listen to one of the group, probably the eldest, "talk old stories." With eyes that show the whites in exclamation, and ejaculations of "O Lawd!" "Go!" "Do now!" etc., long drawn out in pleasure, the younger ones nestle close together so "De Debble" wont get them as he does "B' Bookey" or "B' Rabby" of the story.

These tales are divided into two classes—"old stories" and fairy stories—the former particularly constituting the negro folk-lore, while the latter have been introduced from the same source as the ordinary fairy-tales of English children. It is a curious fact that some of the fairy-tales have been translated, so to speak, into old stories, and one easily recognizes in such a tale as "B' Jack an' de Snake" its English ancestor of "Jack the Giant-Killer."

The folk-lore proper is mostly concerning animals which, personified, have peculiar and ofttimes thrilling adventures. Where, in our own negro-lore, the animals are called "Brer," among the Bahama negroes this term is contracted to "B," and so one finds in "B' Rabby, who was a tricky fellow," the "Brer Rabby" whom Uncle Remus has made famous to us as the hero of the folk-lore of the South.

The conventional negro dialect, used in our American stories, will apply to the Bahama negroes only in part, for their speech is a mixture of negro dialect, "Conch cockney," and correct English pronunciations. In the following stories, which are given as nearly as possible verbatim, this apparent inconsistency will be noticed, for in the same story such expressions, for example, as "All right" and "Never mind," may be given in the cockney, "Hall right," in negro dialect, "Ne'r min'," or pronounced as written in correct English, and one may never know which pronunciation to expect.

In these stories one readily detects the influence of physical environment and the play of native invention, in the predominance given to those animals and plants locally prominent, acting their parts among scenes borrowed from local surroundings. On the other hand, the introduction of the lion, elephant, and tiger suggests an heredity from African ancestors, while similarly the rabbit, in the title rôle of hero, as rabbits are not indigenous in the Bahamas, points to the influence of American negro-lore. The isolation of the Bahamas from foreign influences, the scanty supply of books and newspapers, and the great lack of what are generally termed amusements, has given especially good conditions for the development of a folk-lore at once recognized as peculiar and sectional.

An atmosphere, that paper cannot hold, is added to these tales by the physical conditions;—an island out in the Atlantic, arising with low shores from that indescribable blue water, covered by the paler blue of the skies of "Summerland," heated by the glaring sun of mid-day, or bathed in silver radiance by the queen of night; the querulous gulls, catching fish in the shallows or "white water;" the cunning little lizards which, from orange tree and stone wall, watch your every step; the dazzling white of the streets, and the superficial piety of the people; the sea gardens, where, in the clear water, one beholds the fans and feathers of the sea waving in response to tide and billow; beneath them the creeping stars, the spiny urchins and long cucumbers crawling among the tentacled anelids and anemones, and chasing in and out, above and around these more simply organized creatures, the fishes, banded in gold and black and orange, with long, waving filaments to their fins and high foreheads, which solemnly suggest an intellect only developed in higher forms; and then, finally, those colonies of coral animals, which inhabit the top of a submarine precipice built of the skeletons of their ancestors through millions of generations, and which ere long will die to complete the foundation for another island or series of islands.

There is perpetual beauty on land and in the sea, while the balmy, even-tempered air invites one to sail over the blue waters or to lie in a hammock beneath the palms and listen to some black "Conch" "talk old stories." One boy named Dennis was very much the best story-teller on the cay, and from him I took most of the following tales, but the quick, short gesture, the peculiar emphasis on the exciting words and phrases, the mirth now bubbling from eyes which anon would roll their whites in horror, in short, the *Othello* part of the tales, I cannot give.

II.[1]

B' RABBY IN DE CORN-FIELD.

Once 'twas a time, a very good time,
De monkey chewed tobacco, an' 'e spit white lime.[2]

So dis day, it vwas a man; had a big fiel'; peas, corn and potato. De man didn't used to go in de fiel.' 'E send his boy. So dis day B' Rabby come vw'ere de boy vwas. 'E say, "Boy, you' pa say, gi' me some peas, corn and potato." 'E let 'im eat as much as 'e vwant. De nex' day B' Rabby come back again. 'E say, "Boy, you' pa say, gi' me some peas, corn an' potatoes." So now vw'en de boy vwent home de boy say, "Pa, you tell B' Rabby to say, I say must give 'im peas, corn an' potatoes?" De man say, "No, I aint see B' Rabby." 'E say, "De nex' time B' Rabby come dere you mus' tie B' Rabby an' let 'im eat as much peas, corn an' potatoes as 'e like." 'E say, "You mus'n' let 'im go." De nex' day B' Rabby come. 'E say, "Boy, you' pa say, 'Gi' me some peas, corn an' potatoes.'" B' Boy say, "Le' me tie you up first." B' Rabby say, "All right, but vw'en I done eatin' you mus' let me go." B' Boy say, "All right," too. Now vw'en B' Rabby vwas done eatin,' B' Rabby say, "Boy, le' me go now!" B' Boy say, "No!" B' Rabby say, "Min', you better le' me go!" B' Boy say, "No!" B' Boy vwent to call his pa. B' Boy say, "Come pa, got 'im to-day!" De man vwent over in de fiel'. Dey ketch B' Rabby. Bring 'im up; put 'im in de hiron cage. Now dey had on six big pots o' hot vwater. B' Tiger vwas comin' past. 'E say, "Vw'ats de matter, B' Rabby?" 'E say, "Dey got me in here to marry de Queen's daughter, an' I don' vwan' to marry 'er." Now 'e say, "D' see dem six pots dere? Dey got dem full, *full* o' cow-'eads for my weddin'." B' Tiger say, "Put me in, I vwan' to marry de Queen's daughter." B' Rabby say, "Take me up!" B' Rabby jump up. 'E fasten up B' Tiger. Now B' Rabby *gone!* 'E git in one hollow, hollow pison-wood tree.

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[1] As the purpose of this paper is more to be a record of fact than a literary production, I must again call attention to the fact that there is not an invariable dialect among the Bahama negroes, the same word often being pronounced differently in succeeding phrases or sentences. My effort therefore has been to report these tales phonetically as I heard them, and not upon themes given to write new stories.

[2] The old stories are almost always introduced by this doggerel verse, and very often some expression, as, "Twant my time; twant you' time; was old folks' time;" is added.

De boy come out an' say, "Pa, dey big one here!" De man say, "Don' care if 'e big one or little one, I goin' to scal' 'im. De man come out. 'E git de hot vwater. 'E take de big pot full, *boilin' up*; 'e swash on B' Tiger. B' Tiger, 'e holler, "*Tain' me! tain' me!*" De man say, "Don't care 'f tain,' you or vw'at, I goin' scal' you." 'E scal' B' Tiger. B' Tiger 'e give one jump; 'e knock de cage all to pieces. B' Tiger *gone!*

'E come to dis same pison-wood tree B' Rabby vwas in. 'E sit down right on de stump o' de tree vw'at B' Rabby vwas in. B' Rabby had one sharp, sharp stick, an' 'e shove right into B' Tiger. B' Tiger say, "*My goody!*" 'e say, "Hants here!" B' Rabby take de stick; 'e shove it out; 'e stick B' Tiger. B' Tiger say, "No dis aint hants; B' Rabby here." Den B' Tiger look down in de hole an' 'e see B' Rabby settin' dere. B' Tiger say, "*Ha-an!* B' Rabby! Never min', you cause me to get scalded;" 'e say, "N'er min'! I goin' ketch you!" B' Rabby say, "Move boy! Le' me git out! Doan' min' me!" B' Rabby *gone!*

B' Rabby see one dead goat in de road. De goat dead; stink and be rotten. All de goat back vwas rottin' avay. B' Rabby *gone;* 'e git inside de dead goat.

B' Tiger vwas comin' fas'! Vw'en B' Rabby look, 'e see B' Tiger comin'. B' Rabby vwas doin, "*Huhn! huhn!*" vwas doin' so in de goat, "*Huhn! huhn!*" B' Tiger say, "Vw'ats de matter, B' Goat?" 'E say, "B' Rabby vwen' past here just now; poin' he finger at me an' rottin' avay all my back." B' Rabby *gone* out de dead goat.

B' Rabby vwas fishin.' 'E see B' Tiger. B' Rabby had he finger bent. B' Rabby say, "Min', I goin' poin'!" B' Tiger say, "*Do* B' Rabby, doan' poin'!" Dats vw'at B' Tiger say. B' Rabby didn' poin.'

[1] E bo ban, my story 's en',
If you doan' believe my story 's true
Hax my captain an' my crew.
Vw'en I die bury me in a pot o' candle grease.

B' HELEPHANT AND B' VW'ALE.

Once it vwas a time, etc.

Now dis day B' Rabby vwas walkin' 'long de shore. 'E see B' Vw'ale. 'E say, "B' Vw'ale!" B' Vw'ale say, "Hey!" B' Rabby say, "B' Vw'ale, I bet I could pull you on de shore!" B' Vw'ale say, "You cahnt!" B' Rabby say, "I bet you tree tousan' dollar!" B' Vw'ale say, "Hall right!" 'E *gone*.

'E meet B' Helephant. 'E say, "B' Helephant," 'e say, "I bet you I could pull you in de sea!" B' Helephant say, "Me!" 'E say, "Dey aint ary man in de worl' can pull *me* in de sea!" B' Rabby say, "I'll try it to-morrow at 12 o'clock."

'E gone an' get a heap o' rope. 'E say, "Now to-day we'll try it." 'E tie one rope aroun' B' Vw'ale's neck, and den 'e tie one aroun' B' Helephant's neck. 'E say, "Vw'en you hear me say, '*Set taut,*' you must set taut." 'E say, "*Pull avay!*" Vw'en B' Vw'ale pull, 'e pull B' Helephant in de surf o' de sea. 'E say,

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[1] The first three lines of this doggerel verse form the customary ending of a story, while the last line may be added to suit the individual fancy of the narrator.

"You tink dis little B' Rabby doin' all o' dat!" W'en B' Helephant pull taut, 'e pull B' Vw'ale in de surf o' de sea. B' Vw'ale ketch underneath one shelf o' de rock, and B' Helephant ketch to one big tree. Den de two on 'em pull so heavy de rope broke.

B' Vw'ale went in de ocean and B' Helephant vwent vay over in de pine-yard. Das vy you see B' Vw'ale in de ocean to-day and das vy you see B' Helephant over in de pine bushes to-day.

'E bo ban, etc.

B' RABBY, B' SPIDER AN' B' BOOKY.

Once it vwas a time, etc.

B' Rabby, B' Spider an' B' Booky wen' in de field. As evenin' vwas comin' dey was comin' home in de boat. An' dey had one bunch o' bananas to share an' dey didn' know how to share it. An' B' Spider did say to B' Rabby, "Trow de bunch o' bananas overboar' an' den who could *dive* de mostest could *have* de mostest."

Den dey pull off der close. B' Rabby had de furst dive. Vw'en 'e vwent down to bottom 'e bring up four bananas. Vw'en B' Booky vwent down 'e bring two. B' Spider vw'en 'e pitch overboar' 'e float.

B' Rabby pitch overboar' again an' 'e bring up six. B' Booky pitch overboar' again an' 'e bring up four. B' Spider pitch overboar' again; 'e *float.* 'E say, "You no tie de grapple to me an' le' me go down an' get hall." An' 'e vwent down; 'e ketch hall on 'em; an' 'e couldn' come hup no more.

An' B' Rabby take his knife an' cut avay de rope. An' den dey vwent home. An' B' Spider———; vw'en dey hax 'em, "Whey B' Spider?" An' B' Rabby say, "B' Spider 'e did have such a big eye;

'e did vwant all de bananas an' 'e couldn' dive." 'E say, "B' Spider did say, 'You no tie dis grapple to me an' le' me get hall.'" An' de Spider's mudder say, "'F you don' go fetch 'im I put you in prison." An' dey say dey aint gwine. An' de vwoman did carry 'em to prison. An' B' Rabby did put de vwoman in jail. An' de judge did say, 'e couldn' put B' Rabby in prison fur dat, cause 'twas B' Spider's fault.

E bo ban, my story 's end, etc.

B' MAN, B' RAT AN' B' TIGER-CAT.

Once it vwas a time, etc.

So now dis day; ebry time de rat use' to go in de man' field eatin' de man' peas, potatoes an' his corn. So now dis day de man ketch de rat; 'e had de rat in de cage to kill 'im. De rat say, "*Do,* B' Man, spare my life, I'll never come back any more!" De man say "*Hall* right!" 'E let de rat go. B' Rat vwent vay hover in de vwood; 'e never come back any more.

Dis day de man vwent shootin' pigeons. 'E vwent vay over in de vwoods; 'e shoot a big bunch o' pigeons. Now 'e *gone,* 'e see tree young tiger-cats. De man vw'en' 'e gone 'e take all tree de tiger-cats. Soon as 'e make one step de hold tiger-cat, 'e after 'im an' growl. De tiger-cat say, "Taint no good to put down my young ones so you might as well keep 'em. B' Tiger-cat say, "B' Man, le' me tell you vw'at to do."

B' Tiger-cat vwas hup in one tree ready to pitch on de man. B' Tiger-cat say, "You let de dog heat de pigeons; you heat de dog, an' let me heat you." De man stan' up an' 'e study. B' Tiger-cat say, "Talk fas', B' Man; talk fas'!" 'e say, "Let de dog heat de pigeons; you heat de dog, an' le' me heat you." Den dat same rat w'at de man let go jump out de road. 'E say, "Yes, B' Man, do dat; give de pigeons to de dog; you heat de dog, an let B' Tiger-cat heat you, an' let me heat B' Tiger-cat."

B' Tiger-cat stan' up; 'e study. B' Man say, "Talk fas', B' Tiger-cat, talk fas!" B' Man vwent towards his gun. B' Tiger-cat jus vwas studerin' on vw'at B' Man say. Den de man pick up his gun. Vw'en 'e fire 'e shoot B' Tiger-cat dead. Den B' Rat jump up an' say, "One good turn deserve another! One good turn deserve another! One good turn deserve another!"

E bo ban, etc.

B' BOOKY AN' B' RABBY.

Once it vwas a time, etc.

It vwas B' Rabby; 'e use' to go to de Queen's pasture ebry night an' take de bigges' sheep from de flock. So dis night vw'en' 'e wen', de Queen's servant did put a lion at de head o' all de huder sheeps. B' Rabby vwas takin' dat to be a sheep, an' 'e carried 'im a little vays in de road an' 'e say, "Look 'ere, dis t'ing don' vwalk like sheep, dis t'ing vwalk like lion!" An' 'e call out for B' Booky. 'E say, "B' Booky, 'ere, take dis sheep, I got to go up 'ere in de wood for de huder one I got tied in de fence!" An' vw'en B' Booky get a little vays, 'e sing out, 'e say, "Dis t'ing don' vwalk like sheep!" 'E say, "Dis t'ing vwalk like lion!" Den 'e hollered out to his vwife an' children, tell dem all to get up in de roof o' de house. 'E holler out, say "De lion comin' to tear you to pieces!" An' vw'en de lion get to de house, 'e walk in tr'u' de door an' 'e see all on 'em up in de roof an' 'e look up at 'em.

An' de smalles' chil' say, "Fadder an' mudder, I know you love me, but I cahnt hold out no longer!" An' de fadder say, "See lion 'ere!" An' vw'en de chil' drop de lion tear her to pieces. De huder one say, "Fadder an' mudder, I know you love me, but my harms is tired!" An' his fadder say, "See lion dere!" An' vw'en 'e drop de lion tear 'im to pieces. Dis de bigges' one now; 'e say, "Fadder an' mudder, I know you love me an' I love you, but I cahnt hold out no longer!" An' de fadder say, "See lion dere!" An' vw'en 'e drop de lion tear 'im to pieces. His vwife say, "I know you love me," an' she say, "I love you too!" De husband was so pitiful 'e couldn' talk, an' 'e jus' pint his finger down to de lion an' his vwife drop. An' after 'e see all on 'em vwas gone 'e vwent out tr'u' de roof o' de house an' 'e stay dere until de mornin', an' dats how 'e vwas save. B' Lion couldn' jump an' 'e vwen' avay.

E bo ban, my story 's end, etc.

B' BARACOUTI[1] AN' B' MAN.

Once it vwas a time, etc.

Once it vwas a man; 'e had a fiel' 'pon a differ'n' part o' de shore. Dis day 'e did vwan' to go to his fiel'; 'e met a shark. 'E said to de shark, "Please carry me 'cross to my fiel'!" B' Shark say, "All right!" an' 'e carried him 'cross. Vw'en 'e got 'cross 'e give B' Shark a good cut. B' Shark say, "*All* right!"

'E come out again from his fiel'; 'e meet B' Shark again. 'E say, "B' Shark, *please* carry me 'cross, once more!" B' Shark say, "All right!" An' B' Shark carried him 'cross again. An' 'e give B' Shark a heavy cut again. B' Shark say, "*All* right!"

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[1] The Baracouti is an eel-like fish, with numerous, strong, sharp teeth, and is very savage when attacked.

De nex' day de man did vwan' go 'cross again. 'E say, "B' Shark, *please* carry me 'cross to dat shore;" 'e say, "I'll give you a fortune!" B' Shark carry 'im again, an' 'e give B' Shark *such* a cut, till B' Shark had to lay awake till 'e come out again.

Sun vwas nearly down vw'en de man come out. 'E say, B' Shark, *please* carry me 'cross again;" 'e say, "I'll pay you vw'en I get 'cross." B' Shark say, "Get on my back." De firs' fish B' Shark meet vwas a Corb.[1] B' Shark say, "B' Corb, you do man good an' man do you harm;" 'e say, "vw'at you mus' do to'r' him?" B' Corb say, "Cut 'im in two!" Nex' vwas a Porpy.[2] 'E say, "B' Porpy, you do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Porpy say, "Leave it to God!" De nex' vwas a Baracouti. 'E say, "B' Baracouti, you do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Baracouti say, "Cut 'im to hell!"

B' Shark see B' Rabby on de rocks. 'E say, "B' Rabby, you do man good an' man do you harm, vw'at you mus' do to'r' him?" B' Rabby say, "Come in little further, I ain' hear you!" (B' Rabby vwan' to save de man.) 'E come in. 'E say, "B' Rabby, you do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Rabby say, "Come in little bit further; still I ain' hear you!" B' Shark come in a little bit further. 'E say, "I cahn' come no further else I get 'shore!" B' Shark say, "You do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Rabby say, "Vy, let' im jump 'shore!" Before de Shark could turn 'round to go with 'im de man jump 'shore an 'B' Shark commence to cry.

E bo ban, my story 's en', etc.

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[1] Flat-headed shark, particularly dreaded as a man-eater.

[2] Porpoise.

B' LOGGERHEAD AN B' CONCH.

Once it vwas a time, etc.

Dey vwanted de King's daughter. King told de two to have a race, de one dat beat de race to have his daughter. Dey hask him, "Vw'at sort o' race dey mus' have." 'E said 'e wanted to see who could vwalk de fastes' out o' two. Dat vwas de Loggerhead and de Conch. De Conch knowed dat de Loggerhead could beat 'im walkin' 'so de Conch vwent an' hired hother Conchs an' put 'em to de marks' stake. Den after dat 'e vwent down to de river whe' de Loggerhead vwas en' told 'im 'e's all ready for de race.

'Im an' de Loggerhead started off together. De first mark de Loggerhead get to 'e meet a Conch dere, takin' it to be de one dat 'e start off to race with, but standin' talkin'. De one dat went to race, 'e went ahead o' de Loggerhead. Den de Loggerhead started from the place where de Conch vwas, expectin' it vwas de same Conch. Vw'en 'e git to de nex' pole 'e meet a Conch again still thinkin' it vwas de same Conch. Stand dere dey small-talk; whilst talkin' give de Conch vw'at hired de other Conchs a chance to chat with 'im, den de Conch had chance to go 'is vway. Vw'en de Loggerhead git to King's palace, 'e met de Conch 'head of 'im. De Conch had beaten de race an' 'e got de King's daughter. Den after dat de Loggerhead say 'e vwould take de sea for 'is dwellin' place.

E bo ban, my story 's en', etc.

B' CRANE-CROW, B' PARROT AN' B' SNAKE.

Once it vwas a time, etc.

It vwas a heagle, layin' in a tree. Hafter she had young ones dis snake use to plague de tree. So afterwards B' Heagle lef' B' Crane-crow an' B' Parrot to watch dese young ones, vw'en B' Snake come, to call 'er. So vw'en de snake come, dey call dis heagle Dey say, "Ma hoo heagle! De snake comin'!" So she come. Therefore she kill de snake. She said, "Hafter he het my young ones;" she say, "Therfore I'll go nord, I'll live dere all my life on de nordern part of Baltimore.[1] I no more to say. De snake had het my young ones." Dats makes so you see heagle live hover dere to-day; dey won' come dis side.

B' CRICKET AND B' HELEPHANT.

Once it vwas a time, etc.

So it vwas palm-ile tree whe' dey use' to go to feed. So hevery time B' Helephant use' to go dere, 'e use to meet B' Cricket. 'E say, "B' Cricket, I bet I can mash you up some o' dese days." So B' Cricket say, "B' Helephant, you cahn' mash me up, fur it don' stan' fur de bigness o' man; little man could make big man run." B' Helephant say, "Go vay, B' Cricket!"

So dis day vw'en B' Helephant come to de tree B' Cricket vwas dere. B' Helephant didn' see 'im. 'E vwen' in B' Helephan' ear-'ole an' e' git to singin,' an' B' Helephant 'e did put off a runnin'. Everywhe's 'e put 'is foot it vwas river. So 'e meet B' Lion. 'E say, "B' Lion, man' mo'n me to-day;" 'e say, "I 'bout de bigges' beast in de fores' an' you 'bout de stronges'; an' still, if you vwas to hear vw'at I hear to-day, make you run."

"Let 'im come, I'm a man fur anything," dats de word B' Lion say. B' Cricket jump out B' Helephan' ear-'ole an' gone in B' Lion ear-'ole, an' vw'en B' Cricket sing out in B' Lion ear-'ole, 'im an' B' Helephan' start together. B' Helephan' see B' Lion runnin', taught de soun' o' de cricket vwas still in 'is ear-'ole. Some iron-wood tree dere vwas six times big as dis house. B' Lion tear 'em right square up by de root.

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[1] To the people of Green Turtle Cay, Baltimore, from whence come the schooners in the pine-apple trade, is one of the great places beyond the sea of which the children, especially, have peculiarly vague ideas.

Vw'en dey get dere dey meet B' Jack standin' on de hill. Jack say, "Vw'at you no runnin' 'ere 'bout?" Dey say, "B' Jack, man 'ere to-day mo'n you an' me an' you two together!" B' Jack say, "I'm de man to heat you an' de man too!" So B' Cricket jump out de lion ear-'ole an' vwen' in B' Jack own to tell 'im de hargemen' B' Helephan' an' 'im had under de palm-ile tree. So B' Helephan' 'e quiver so much 'e drop down dead. B' Jack say, "My deah man, dat vwas de harge vw'at you an' B' Cricket had." B' Cricket say, "I tell you 'bout a little man every day;" so a puff o' win' come an' end dis story.

E bo ban, etc.

B' JACK AN' B' SNAKE.[1]

Once it vwas a time, etc.

De Queen say, "B' Jack, if you can kill dis snake, I don' know how much money I wouldn' give you!" So Jack say, "I wan' five hundr' dollars to go on a spree." So 'e gone up dere now. 'E say, "B' Snake, vw'at you tink dese foolish people say; dey say you' body cahn' go in dis half-hitch." 'E come out de hole an' 'e vwen' in de half-hitch. Den Jack draw de half-hitch taut. Den all dese soljers come around; dey cut 'im up in pieces.

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[1] Probably founded upon "Jack the Giant-Killer."

So den de Queen say, "Jack, I got one more trial fur you to do." 'E say, "Vwell, vw'at dat is?" "If you could go up 'ere in dis corn fiel' an' kill all de rice-bird, I let you git married to my daughter," dats vw'at the Queen say. 'E vwen' to de fiel' to de rice-bird. So B' Jack say, "B' Rice-birds, vw'at you no tink dese foolish people say; dey say all o' you no rice-birds cahn' full up dis basket!" De rice-birds say, "Vy people so foolish, no all us rice-birds cahn' full up dat basket!"

So all de rice-bird vwent in de basket. So B' Jack drawed de basket together with de rice-birds in it. So B' Jack vwen' home to de Queen with dese rice-birds. Say "Her' de rice-birds." Queen say, "Vwell, B' Jack, you can get married to my daughter."

E bo ban, etc.

B' CRANE-CROW AN' B' MAN.

Once it vwas a time, etc.

Now dis day the Queen did vwant a man to see if 'e couldn' ketch dis Queen Crane-Crow. De man gone whey all de Crane-Crows use' to come. Now de man lay down an' make believe 'e vwas dead. Now hall de Crane-Crows come. All on 'em vwas singin'. Crane-Crows vwas say'n, "Hunte man dead to-day; Hunte man dead to-day." Dis Queen Crane-Crow say, "Save 'is eye-ball fur me!" Dey didn' vwan' believe 'e vwas dead. 'E sen' one o' de hudder Crane-Crows to pick 'im.

Dis little Crane-Crow *gone*, 'e pick de man. B' Queen Crane-Crow say, "Pick 'im again!" 'E pick 'im; de man ain' move. Now hall on 'em vwas comin'; begin to pick de man. Vw'en dis Queen Crane-Crow come to pick hout de man heye; de man hold de Queen Crane-Crow. De Queen Crane-Crow holler, “Tain' me! Tain' me! Tain' me!” De man say, “No good, I got you now, you got to go.” 'E put 'im inside 'e bag; 'e carry 'im to de Queen. De Queen give 'im a fortune an' de man vwas rich fur 'is life time. (Dats hall.)

E bo ban, dat story 's en', etc.

De Big Worrum.

Once it vwas a time, etc.

So dis day it vwas a man; he had two sons; dey didn' have no fire. Hall dey had to heat vwas raw potatoes. Now de man sen' dis boy to look for fire. De boy vwalk; he vwalk; he vwalk till vw'en 'e look 'e see one smoke. Vw'en 'e gone 'e git to dat fire. Vw'en 'e get dere, de worrum vwas full o' fire. De boy say, “Dimme some fan!” (Give me some fire). De worrum say, “Tain', tain' none; jes do fur me.” De worrum say, “Come in little closer ” *Good!* Soon as de boy vwen' a little closer, vw'en 'e vwent to reach de fire de worrum swallow 'im down. Den de boy vwen' down, right down, down inside de worrum till 'e stop. De boy met whole lot o' people vwat de worrum did swallow.

So now the man tell de hudder son, “I wonder whey my son gone!” De hudder son say, “Pa, I goin' look fur him.” 'E vwalk, 'e vwalk, 'e vwalk till 'e come to this big worrum vw'at had de fire in his mouth. So now the boy vwent to de worrum. De boy say, “Dimme some fan!” De worrum say, “Keelie o' fire” (Come and get fire). De boy say, “*Do i en e*,[1] dimme some fan?” De worrum say, “Come a little closer.” De worrum say, “Time for

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[1] A phrase, the original meaning of which, had it any, is lost.

Joe come" (Time to go home). De worrum say, "Keelie o' fire." Vw'en de boy vwen' to get the fire *so*, de worrum swallow him down. De boy vwen'; 'e vwen' down; 'e vwen' down, till 'e met 'e brudder.

Now de boy fadder say, "My two sons gone an' I might as vwell gone too." De man take 'e lan' (lance); it fairly glisten, it so sharp. Vw'en 'e git dere whey de worrum vwas wid de fire in he mouth, de man say, "Dimme some fan?" De worrum say, "You too do fur me!" (You're too much for me). De worrum say, "Keelie o' fire." Vwen de man vwen' to get de fire, *so*, de worrum vwen' to swallow 'im. De man take he' lan'; as 'e vwas goin' down 'e cut de worrum; 'e cut de worrum till 'e cut de worrum right open an' all de people come, an' dat vwas a big city right dere.

E bo ban, dis story 's en', etc.

ON SOME CHARACTERISTICS OF SYMBOLIC LOGIC.

CHRISTINE LADD FRANKLIN.

It is now thirty-five years since the publication of Boole's *Laws of Thought*. It has frequently happened, of course, that a great advance in the work of extending scientific ways of thinking over regions which are still in the hands of the natural man has failed to meet with the recognition that it deserved, but, none the less, every fresh instance of a misfortune of this kind is a fresh subject for regret.

The task which Boole accomplished was the complete solution of the problem:—given any number of statements, involving any number of terms, mixed up indiscriminately in the subjects and the predicates, to eliminate certain of those terms, that is, to see exactly what the statements amount to irrespective of them, and then to manipulate the remaining statements so that they shall read as a description of a certain other chosen term (or terms) standing by itself in a subject or a predicate. Ordinary syllogism consists of elimination in the simplest possible case; when we infer from "all kings are tyrants," and "all tyrants are assassinated," that "all kings are assassinated," what we do is to gather up all the information which is conveyed by both premises together, exclusive of that which concerns tyrants. It is plain that ordinary syllogism is of very little avail when there are several premises of such complexity as this, for instance:—"Every a is one only of the two, x and y, unless it is z or w; in the former of which cases it is both x and y, and in the latter case neither of them.

However great a distaste one may have for Symbolic Logic, and however certain one may be that such complicated states of things as the above sentence describes never arise in nature, it is impossible not to admit that it is a great gift to the powers of the common mind to have devised a method by which one can sit down before half a dozen premises of this kind and know just how to go to work to pick out with absolute certainty everything that is said about anything, with or without allusion to anything else. The unaided mind, with a good deal of struggling and floundering, can do pretty hard work of this kind, if it has unusual powers of concentration, but it cannot do it without extreme effort. An organized method for doing thinking like this (which is simply what Symbolic Logic is) amounts, to say the least, to an immense economy of intellectual work. This problem of Logic was completely solved by Boole.

The underlying process of thought involved in his rule for elimination consists, as might have been predicted, in nothing more nor less than the syllogism; the mental act by which, when a term is so given as to form a safe connecting link between other two terms, that term is dropped and the implied connection between the other two terms is set forth in a single statement, is syllogism and nothing else. The only difficulty which the natural man need feel in cases of any complexity whatever, is to extricate the term to be eliminated from the mass of terms attached to it either conjunctively or disjunctively, and to set it forth by itself in the subject or in the predicate; if he could accomplish that, an ordinary syllogism would suffice to put it to flight. It is this extricating which ordinary logic refuses to concern itself with, and it is this limitation of its scope which is largely the cause of the unreality and of the remoteness from actual thinking that ordinary logic has in the minds of those who are forced to study it. In everyday life, we have constant occasion for seeing (and we have no difficulty in seeing) the exact equivalence of two such statements as these:

"All students of chemistry are also students of either biology or physics."

"Students of chemistry who do not study physics all study biology."

And yet this is an inference which ordinary Logic takes absolutely no account of. What it amounts to is the changing of a positive disjunctive term in the predicate into a negative conjunctive term in the subject. It is a very simple step for the human mind to take, and yet it is the starting point of the immense command over intricate reasoning which is furnished by the modern developments of Deductive Logic. Common logic itself would gain immensely in scope and freedom, and hence in apparent naturalness, if this process were added to the subjects which it now discusses.

The secret of the great command which Symbolic Logic has over complicated trains of reasoning is wholly contained in the fruitful idea that subject and predicate are not necessarily indivisible wholes, but that they can be broken up and their separate elements shifted at pleasure from one side of the copula to the other. If it were doubted before, by the rigid adherents of Boole, that this is the real essence of the matter, and that the symbolism is merely a convenient, not an essential, tool, it can no longer be doubted since Mr. Keynes (*Studies and Exercises in Formal Logic*) has taken the trouble to write out the whole subject with almost no symbolism at all,—with nothing, in fact, except the absence of a symbol to denote *and* (that is, he writes *a b* for *a and b*), and the use of large and small letters to denote positive and negative terms respectively. Any considerable amount of symbolism is therefore not a necessity to the new logic, but it is, all the same, of an immense advantage to it.

But with the introduction of symbols, there suddenly arises a strong feeling of abhorrence in the mind of the regular logician. They change Logic into a branch of mathematics, he says,—and with the suppressed premise that mathematics is something not to be endured. But the charge is a wholly unfounded one. Whether a thing is mathematics or not, depends upon its subject-matter, not upon the accidents of its dress. Every form of deductive reasoning is mathematical in the sense of being highly abstract, and of being subject to formal rules of procedure, which make it possible to carry it on without knowledge of what the things are that one is reasoning about, but it is non-mathematical in the sense that it does not deal with any kind of quantity. It is merely a question of convenience of nomenclature whether one defines mathematics to be that branch of logic which deals with measurable quantities, or logic to be that branch of mathematics which deals only with objects in masses and their qualities, without regard to their number or their size, or whether (what is doubtless preferable) one considers them to be parallel branches of science in general. In any case, the greater or less extent to which the invariable rules by which one *must* reason are shadowed forth by formal rules for the combination of counters which stand for the products of thought, has nothing to do with the question. It happens that mathematics is a subject of such tremendous complexity that it is absolutely necessary for it to have recourse to all sorts of signs and symbols and abbreviations, in order to make it at all possible for the mind to grapple with it. It does not follow that every other science which finds its advantage in similar means is by that reason turned into mathematics. Modern chemistry makes use of a far more highly developed symbolism than any logician has ever thought of proposing. If the symbolical representation of chemical compounds had happened to receive a distinct name borrowed from mathematics, if it had been called chemical quaternions, for instance, there is no doubt that it would have awakened much repugnance in the minds of conservative chemists, and that it would have had a long struggle to get itself accepted.

What is a symbolic treatment of a subject? It is nothing but a system of abbreviations, having more or less of a pictorial quality, having more or less the character of *icons*, as Mr. Pierce would say, according as that character may be desirable or attainable. Even language is constantly simplifying itself by making use of abbreviated marks for complicated concepts; we said at one time *a moving mass of men;* then we took a single Latin word, *mobile,* for that complex idea; then we abbreviated that word into *mob,* and at present the word mob is, as far as our ordinary consciousness is concerned, nothing but an arbitrary mark for the longer phrase. The great aim which language and science both have always before them, is to enable us to do the greatest possible amount of thinking with the least amount of trouble consistent with perfect clearness. The first aid which the overburdened mind seized hold upon, when the premises thrust upon it were too involved for its easy grasp, was paper and pencil; and it is quite conceivable that some early purists objected to this substitution of a mechanical device for the noble work of reason. It is certain that the early introduction of the signs and symbols of algebra met with very great opposition. It was long before mathematicians were able to reconcile themselves to writing y^5 for $y\ y\ y\ y\ y$; and it was still longer before they ceased writing $y\ y$ instead of y^2; they said, doubtless, that in the case of the second power, y^2 was just as long in writing as $y\ y$, and that the latter was easier of comprehension. But this was at a period when the human mind was not accustomed to bold devices for facilitating its hold over nature. At present, no true lover of science hesitates to avail himself of any well devised scheme for enabling him (with paper and pencil before him) to condense into a single field of vision as much information as possible. And it is very little creditable to the logicians that they have shown an unreasoning horror of a given macroscopic arrangement, merely because it has shown itself to be serviceable in the hands of the mathematicians.

The logicians, it is true, have had some excuse in the fact that the scheme of symbolical reasoning proposed by Boole was immensely more complicated and involved than there was any occasion for. Great as his contribution to science was, Boole had an exaggerated idea of its mysterious inward significance. If he had made less of it, others would without doubt have made more of it. The complete solution of the problem is in reality extremely simple, both in theory and in mechanical execution. The introduction of "functions" and "developments" and the whole idea of inverse processes, while it formed a natural way of looking at the question for the trained mathematician, was wholly unnecessary, and was well calculated to frighten away the mere logician. The whole difficult procedure of Boole has been rendered superfluous by the simpler systems of later writers.

This is not the opinion of Mr. Venn,— the most voluminous of recent writers on the subject. He says (*Symbolic Logic*, p. XXVIII): "Other writers have spoken of their 'systems' and contrasted them with that of Boole; but at present there is, I think, only one thing before the world which can without abuse of language be called a system (unless the single methodical alteration of marking alternatives on the non-exclusive plan be allowed to rank as such), and this is exclusively due to Boole."

In order to make it plain that this is a mistaken view, it is necessary to inquire a little more minutely into what it is that constitutes a system of logic. The inventor of a system of Logic has three distinct tasks to perform:

1. He must take the multitudinous propositions which are handed in to him by common language and reduce them all to a limited number of forms of expression.
2. He must lay down the rules in accordance with which several of these statements are to be united into one, or one is to be broken up into several.
3. He must lay down rules in accordance with which information in regard to some of the terms is dropped while absolutely all the information which does not regard those terms is retained.

These three processes may be called, respectively, expression, combination and elimination. (If any peculiar and unnatural form of expression is to be given to the conclusion — as is the case in Boole's system — that would constitute a separate process.) Expression will consist of two parts: a form must be chosen for putting together the separate terms which go to make up the subject or the predicate, and a form must be chosen for attaching the subject and predicate to each other. Terms are put together in actual thinking in two totally different ways. I mean two different things according as I say:

"All of my friends are *either* learned *or* virtuous." (1)

Or, "All of my friends are *both* learned *and* virtuous." (2)

It is therefore absolutely necessary to have two different signs for connecting the letters, which are to serve us as symbols for terms. These two signs might be anything whatever; the two that are usually employed are, respectively, *the sign of* $+$, and *the absence of any sign*. These are the signs which the mathematician has appropriated to addition and multiplication of quantities, but their use in logic has only a fictitious connection with their use in mathematics. A very large amount of very useless discussion might have been saved if non-mathematical signs had been employed for logic from the start. It is true that in logic a factor is distributed over a sum, as it is in mathematics,

$$a\,(b + c) = a\,b + a\,c,$$

(it is natural to borrow the names *sum*, *factor*, *product*, having borrowed the signs); but so also is a summand distributive over a product,

$$a + b\,c = (a + b)\,(a + c),$$

which is not the case in mathematics. The suggestiveness of the use of the mathematical signs is fully equalled by its deceptiveness.

Nor would the slightest difficulty in the working of the logical algebra be occasioned if the meanings of the signs were interchanged, and if we wrote $a\,b$ for what is either a or b, and $a + b$ for what is both. But the fact remains that there is hardly anything which is easier to write than $+$, and nothing which is easier to write than nothing; and also that to write $a\,b$ for what is both a and b, while it has absolutely no connection with multiplication, does closely simulate the device of grammar by which we say *green apples* to indicate those things which have both the qualities of apples and the quality of being green. But whatever two pictures might be chosen to stand for these two kinds of term-attachments, *the rules for their manipulation must always be exactly the same.* Two statements which are identical in meaning, respectively, with the two statements given above are:

"Whoever is *both* unlearned *and* non-virtuous is not my friend." (1)

"Whoever is *either* unlearned *or* non-virtuous is not my friend." (2)

Thus, a conjunctive combination in the predicate of the affirmative proposition becomes disjunctive (and of opposite quality) in the subject, and conversely. In other words, the negative of a sum is a product of negatives, and the negative of a product is a sum of negatives; or, expressed symbolically, $\overline{a + b} = \bar{a}\,\bar{b}$, and $\overline{a\,b} = \bar{a} + \bar{b}$; that is, what is not either a or b is both non-a and non-b, and what is not both a and b is either non-a or non-b. This rule is an immediate consequence of the two properties of the negative (namely, that x and $\bar{x}$ are mutually exclusive and that they together exhaust the universe); and it must appear in exactly this form in every system of logic (regard not being had to the unimportant modifications produced by marking alternatives on the exclusive plan).

Now if a system of logic consisted in the treatment of the aggregation and determination of terms alone, it would be correct to say that Boole's system is the only one that exists; but in that case Boole's system could not be credited to Boole. Mr. Venn has himself pointed out that Lambert (*Logische Abhandlungen*, 1781) described these logical relations by these words and represented them by the signs + and ×.

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FOUR DIFFERENT STATEMENTS OF FACT.

	NON-SYMMETRICAL.		SYMMETRICAL.	
UNIVERSAL.	(A) All of $\underline{a}$ is b. All of $\bar{b}$ is $\bar{a}$.	(∀) None but $\underline{a}$ is b. None but $\bar{b}$ is $\bar{a}$.	(E) None of a is b. $(ab)_0$	(Ǝ) All but a is b. $^{\infty}(a+b)$
PARTICULAR.	(a) Not all of $\underline{a}$ is b. Not all of $\bar{b}$ is $\bar{a}$.	(ɐ) Some besides a is b. Some besides $\bar{b}$ is $\bar{a}$.	(e) Some of a is b.	(ɘ) Not all but a is b.

I have elsewhere suggested copula signs for these eight propositions, which indicate by their several characters whether the proposition which they symbolize is universal or particular, positive or negative, symmetrical or non-symmetrical; and which are such that a simple rule shows how to transform a given proposition from one form into any other.[1] It will be noticed that the four propositions of the right-hand half of the table can be read backwards as well as forwards,—"all but a is b" is the same thing as "all but b is a;" but that is not the case in the left-hand half of the table,—"none but a is b" is not the same thing as "none but b is a," but it is the same thing as "none but $\bar{b}$ is $\bar{a}$," or, on changing the places of the terms their quality must be changed also, in order to get an equivalent statement. We shall express this by saying that the copulas of the left-hand half are non-symmetrical and those of the right-hand half are symmetrical. Of the symmetrical copulas, one can be inserted anywhere in a product, and the other can be inserted anywhere in a sum; "none of $a\ b$ is $c\,(b+d)\ e$" is identical with "none of $a\ b\ c\ (b+d)$ is e," and "all but a is $b\ c+d\ e\ f$" is identical with "all but $a+b\ c$ is $d\ e\ f$," with the same thing for the corresponding particular propositions. Moreover, with the symmetrical forms of expression, nothing is changed by putting all the terms into the subject or the predicate as the case may be, thus: "no a is b" is the same thing as "a which is b is nothing," and may be written, upon occasion, $(a\ b)_0$; and "all but a is b" is the same thing as "everything is either a or b," and may be written, upon occasion, $_\infty(a+b)$.

The table already given exhibits the four different forms of expression for four different statements of fact. The four universal (or the four particular) copulas may be made to give expression to one and the same fact by attaching the proper quality to the terms which enter them. Thus, in the following table, all of the universal propositions are equivalent in meaning with "no a is b," and all of the particular propositions are equivalent in meaning with "some a is b."

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[1] Some Proposed Reforms in Common Logic. To appear in *Mind*, January, 1890.

FOUR DIFFERENT FORMS OF EXPRESSION FOR ONE AND THE SAME STATEMENT OF FACT.

	NON-SYMMETRICAL.		SYMMETRICAL.	
UNIVERSAL.	(A) All of a is $\bar{b}$. All of b is $\bar{a}$.	(∀) None but $\bar{a}$ is b. None but $\bar{b}$ is a.	(E) None of a is b. $(a\ b)_0$	(Ǝ) All but $\bar{a}$ is $\bar{b}$. $\infty(\bar{a} + \bar{b})$
PARTICULAR.	(a) Not all of a is $\bar{b}$. Not all of b is $\bar{a}$	(ɐ) Some besides $\bar{a}$ is b. Some besides $\bar{b}$ is a.	(e) Some of a is b.	(ə) Not all but $\bar{a}$ is b.

In other words, the fact, "no a is b," can be expressed, at pleasure, in terms of a and b, $\bar{a}$ and $\bar{b}$, a and $\bar{b}$, or $\bar{a}$ and b; and the same is true of the fact, "some a is b."

But the first requirement of a good working scheme is that it should admit no more variety of expression than is absolutely necessary. A single fact, instead of being expressed, at pleasure, in four different ways, as is done in real life, must be expressed in one way only. *Now the whole subsequent working of a scheme of logic will be totally different according as one or the other of these four different modes of expression is chosen as the normal form.*

For one thing, if all propositions are expressed in either of the symmetrical forms, there will be no distinction between subjects and predicates, and both will be treated in accordance with exactly the same rules; but if either of the non-symmetrical forms is chosen, the rules for subjects and predicates will be, throughout, the reverse of each other. Take, for instance, the rules for under-statement. From "all of $a + b$ is $c\,d$" it can be inferred that "all of $b\,x$ is $c + y$" (where x and y are anything whatever), but not that "all of $a + b + y$ is $c\ d\ x$;" and the same thing holds for the corresponding particular proposition, "not all of a is b," or "some of a is not b." The rule is, that, with this copula (and its negative) an addend can be dropped or a factor can be inserted in the subject, and a factor can be dropped or an addend inserted in the predicate,—a difficult rule to remember. With the other non-symmetrical copula, the rule is exactly the opposite of this. With either of the symmetrical copulas, only one thing has to be borne in mind for the whole complexus of terms. The combination of two universal propositions is got by adding their terms if the 0-copula is used, and by multiplying them together if the ∞-copula is used; to say that there is no $a\ b$ and at the same time there is no $c\ d$ is the same thing as to say that $(a\ b + c\ d)_0$ but to say that everything is either a or b and at the same time everything is either c or d is to say that ${}_{\infty}(a + b)(c + d)$. With the non-symmetrical copulas, statements cannot be combined at all (without loss of content) except by virtual transformation into one of the symmetrical copulas, unless they happen to have like predicates or like subjects. The process of elimination presents corresponding differences of treatment with different copulas.

It is plain that the several copulas are as unlike as light and darkness. The symmetrical copulas have the immense advantage, in point of simplicity, that all the terms which they connect play the same role. The second non-symmetrical copula is merely the obverse of the first and does not invite separate discussion. The first, "all a is b," has the important consideration in its favor that the propositions presented by nature to the logician are most frequently already in that form. This fact lends an apparent naturalness to this form of expression, and if only questions of moderate difficulty are to be attacked, it might be regarded as a determining consideration. The rules for working with this copula ought to be laid down in any treatise on the subject, because they are the generalization of the affirmative syllogism in the first figure, and the first figure has always been the peculiar favorite of the logician. But when the premises are so complicated that a great deal of transposing between subject and predicate has to be done anyway, it is unwise to strain at the slight additional unnaturalness involved in putting all the terms into the subject or into the predicate, in view of the large amount of mental energy that is set free by the fact that one no longer has to bear in mind a different mode of treatment for terms, according to the place in which they occur.

When Mr. Venn said that there is only one system of Logic, he seems, from a foot-note, to have had in mind only Jevons as a pretended maker of another system; and Jevons's work in Symbolic Logic does certainly not amount to a system, but merely to the absence of a system. Since then Mr. Keynes has published his treatment of the non-symmetrical copula, and he states in his preface that he is peculiarly indebted to Mr. Venn—"not merely by reason of his published writings, but also for most valuable suggestions and criticisms given to me while this book was in progress." From this it might, perhaps, have been inferred that Mr. Venn is now aware that the most difficult problems of Symbolic Logic can be solved with other copulas than the negative symmetrical one, were it not that his latest utterance on the subject is this (*Empirical*

Logic, 1889, p. 230): "In fact, groups of really complicated propositions cannot easily be combined, and their net result completely determined, on any other scheme yet worked out." This, it is true, is a much more guarded statement than his former one, and it is also true that complete combination is less easily effected with either of the non-symmetrical copulas than with the others; but the two symmetrical copulas are exactly on an equality as respects facility of complete combination, and the Logic of the affirmative one was completely worked out, six years ago, by Mr. Mitchell. Mr. Keynes also gives (without noticing its importance) all that is really necessary to the Logic of (universal) propositions beginning in "everything is."

Since the character of a system of Logic is absolutely determined by the character of the copula which is chosen to represent its propositions, it follows that there are four essentially different systems of Logic possible,—provided the form chosen for the expression of the particular proposition is the simple denial of the universal, that is, provided one were always to combine two propositions taken from the same column of the Table. But this is not necessary — it may happen that some advantage is gained by putting together a particular of one couplet and a universal chosen from another, and, as matter of fact, it has happened that a very great advantage has been gained by that means. With this degree of freedom, there are sixteen possible systems of Logic, namely, any one of the four universal propositions, in combination with any one of the four particular propositions, may be taken as the standard form of expression. Have all of these sixteen forms of Symbolic Logic been worked out? Virtually, they have,— or, at least, all which could be of any interest,— and it is therefore no longer a possibility for any one to invent a new system of Logic.

1. The copula chosen by Boole was the negative symmetrical copula, "no *a* is *b*," or "there is no *a b*," which he wrote $a\,b = 0$. This statement is really of the nature of a proposition, "*a* which is *b is* non-existent," not of an equation, and it happens that the equational form of expression, while not close to the real nature of the thing to be expressed, had the unfortunate effect of making people think the subject more mathematical than it was, and hence more unpleasant. For a particular proposition, Boole took the immediate denial of the universal,—"some *a* is *b*;" but the Logic of the complex particular proposition, and of its combinations (conjunctive and disjunctive) with the universal proposition, he did not work out at all. Boole's further method of procedure is deserving of great credit as a first attempt, but it is cumbrous and (to the non-mathematician) mysterious to the last degree. The final form has been given to the Logic of this copula by Schröder,[1] and his treatment of the subject ought to have completely superseded that of Boole. That it has not done so is doubtless owing to the accident that he has not yet had an English commentator. Mr. Venn makes some allusion to Schröder, but only to his adoption of the non-exclusive plan for logical addition, which is an unessential, though praiseworthy, feature of his method. It is very singular that Mr. Venn does not seem to be aware that the problem of Logic has been completely and admirably worked out by Schröder. His rule for elimination amounts to exactly the same thing as Boole's, from the necessity of the case, and yet Mr. Venn can say, of Boole's formula for elimination, that it "does not seem to be thus capable of introduction [*i. e.* with

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[1] Der Operationskreis der Logikkalkuls. Leipzig, 1877.

non-exclusive addition], except under restrictions which almost amount to doing without it altogether." Schröder has accomplished in a few pages, and with admirable simplicity and closeness to real processes of reasoning, what Boole made a very obscure and tortuous journey of. The teacher of Logic who still thinks it necessary to expound the laborious methods of Boole, for any other than historical purposes, is doing his students a serious injury.

I have added the logic of the corresponding particular proposition[1] to Schröder's development of the logic of "no *a* is *b*." The subject is left in a very incomplete state as long as the particular proposition remains undiscussed. Mr. Keynes says (*Formal Logic*, p. 313), "Particular propositions are not in themselves of great value; and they may involve us in troublesome questions of 'existence'." But a universal proposition cannot be simply denied except by means of a particular, and that would be a singular world of argument in which no one was ever permitted to enter a discussion who wished to deny anything which had once been said. Particular propositions do not, in real life, often give us information which is interesting in itself, from a scientific point of view; but they do constantly give us information which is of very great importance in guarding us from a belief in universals which had been supposed to be true. Whoever had been in danger of basing conduct on the premise, "all Catholics are followers of Anti-Christ," would, logically, be very much influenced by the discovery that "some Catholics are saints."

Nor is there anything necessarily troublesome in the question of existence. The import of the particular proposition, "some *a* is *b*," is to affirm the existence of the compound *ab*, and hence of its elements, *a* and *b*. Particular propositions, therefore, always affirm the existence of their terms (or of an alternation of their terms) when expressed in this way. The import of the universal proposition is, in every case, to deny the existence of a compound, but not necessarily to affirm the existence or the non-existence of its elements. If, in a particular case, it is known, by extraneous considerations, that the existence of the subject is actually implied, that fact can be stated (and reasoned upon) as a separate proposition,—namely a particular proposition, an affirmation of existence, "there are some *a*'s." All the consequences of these conventions in regard to existence are perfectly easily carried out, and any algebra, therefore, which does not provide for the introduction of particulars,—that is, which does not permit the denial of universals—is thoroughly and unnecessarily incomplete.

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[1] Studies in Logic, by Members of the Johns Hopkins University. Boston, 1883, pp. 17–71.

The copula "no *a* is *b*" might have been combined with any one of the three other particular forms of expression. Its combination with "not everything but *a* is *b*," instead of with "some *a* is *b*," would give rise to an algebra which would be the exact dualistic of Mr. Mitchell's algebra,—or, it would be exactly like it, if the *meanings* of the symbols for addition and multiplication were interchanged. Its combination with either of the non-symmetrical copulas would lead to much confusion in rules, without any compensating advantage. The syllogisms,

No *x* is *y*.
[illegible]
[illegible]

and

[illegible]
[illegible]
[illegible]

[illegible] the rules for elimination with the com- [illegible]

2. The logic of the non-symmetrical affirmative copula, "all a is b," was first worked out by Mr. Maccoll[1]. Nothing is stranger, in the recent history of Logic in England, than the non-recognition which has befallen the writings of this author. The fact that his contributions appeared in a journal which logicians were not in the habit of referring to (his brief article in *Mind* did not do his method justice), the fact that he was not acquainted with the writings of Boole, and the further accident that he considered it a matter of importance to read "all a is b," which he wrote $a : b$, in the words, "the statement that a thing is a implies the statement that it is b,"—all doubtless contributed to making him seem foreign to writers trained in the usual schools of logic. The fact that the nature of the connection between the terms in "all x is y," and between the propositions in "a is b is-always-followed-by c is d," is exactly the same, and must be exhibited in the same formal rules of procedure, has nothing to do with the *words* in which the proposition and the sequence may be expressed. But if it had not been for this accidental misfortune, it seems incredible that English logicians should not have seen that the entire task accomplished by Boole has been accomplished by Maccoll with far greater conciseness, simplicity and elegance; and, what is an interesting point, in terms of that copula which is of by far the most frequent use in daily life.

Mr. Keynes has re-written the entire logic of this copula, (with the unimportant modification that he prefers to use the printed word *or* instead of the sign $+$), *without so much as a single allusion to Mr. Maccoll.* Mr. Venn says (*Symbolic Logic*, page 372, foot-note): "After a careful study [of this scheme], aided by a long correspondence with the author, I am unable to find much more in it than the introduction of one more scheme of notation to express certain modifications and simplifications of a part of Boole's system." But Mr. Maccoll has completely solved the problem of logic,—to throw the multiform propositions of real life into a single standard form of expression, to condense the information that interests us by the elimination of certain terms which we do not care for, and to state the information which is left in the form of any terms which we happen to wish to see described. The part of Boole's Logic which Mr. Maccoll does not discuss is the inverse operations; but they are an entirely unessential part of the scheme. If anyone cares to interest himself in them, as a matter of idle curiosity, it should be done in an appendix or a foot-note; even in Boole they work merely to obscure the real process of thought, and the actual reasoning, whether formal or not, goes on its way, entirely oblivious of their existence.

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[1] Proc. London Math. Soc., Vols. IX., X., XI., 1877–80.

Mr. Maccoll chooses for the expression of his particular propositions the simple denial of his universals, and he writes them, very properly, with the sign of negation attached to the affirmative copula; but he does not discuss their treatment in cases of any complexity. Neither does Mr. Pierce, who has worked out independently the Logic of the same copula—at least, his Fourth Process [1], which is the rule for elimination, concerns only universal propositions, and his Sixth Process is not true of both, unless "taken together" were to mean *taken in combination if they are universal and taken in alternation if they are particular*. The combination of the universal "all non-*a* is *b*" with "not only *a* is non-b" as its denial, would give rise to an algebra in which the rules for elimination between a universal and a particular proposition would be exactly the same as for that between two universals. This universal combined with either of the symmetrical particulars would not be interesting.

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[1] Am. Jour. of Mathematics, Vol III. (1880), p. 39.

3. The negative non-symmetrical copula, "none but a is b," would give rise to an algebra in which everything would be exactly the reverse of the algebra of "all a is b."

4. There remains the algebra of the affirmative symmetrical copula,—"all but a is b," or, "everything is either a or b." This was admirably worked out by Mr. Mitchell, the late Professor of Mathematics in Marietta College, when a fellow in the Johns Hopkins University[1]. His algebra would have been exactly like the algebra of "no a is b," with the exception that everywhere addition and multiplication were interchanged, had he not had, for the first time, the extremely happy idea of combining with his universal proposition a particular taken from a different rubric,—namely, of working out the algebra of the combination,

Everything is a or b,
Something is $\bar{a}$ $\bar{b}$.

By this device, the rules for combination and elimination (where they can be effected at all) *are all identically the same, whether the propositions are universal or particular.* This circumstance gives this algebra a very great advantage over any which seeks to combine a universal proposition with its immediate denial as the particular. It is an advantage which is shared by the combination.

No a is b,
Not all but $\bar{a}$ is $\bar{b}$,

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[1] Studies in Logic, pp. 72–106.

and, for simple propositions, this would work about as well as that; but for compound propositions, which we now proceed to consider, it happens that this latter couplet of forms of expression is decidedly unnatural.

The four types of universal compound proposition, when completely expressed, are (if P and Q stand for propositions),

A. If *P* is true, *Q* is true,
∀. Only when *P* is true is *Q* true.
E. Never when *P* is true is *Q* true.
Ǝ. Unless *P* is true, *Q* is true,
or, 'The possible' implies that
P is true or that *Q* is true.

A and *E* are the commonest forms of speech for simple propositions, but, by a very curious accident of language, it is not *A* and *E* but *A* and *Ǝ* that are the chosen forms for compound propositions, or sequences, as they may be called, and so great is their preponderance that we have an elliptical form of speech for them,—" if *P*, *Q*," and "either *P* or *Q*," (that is, " if *a* is *b*, *c* is *d* " and " either *a* is *b*, or *c* is d.") They are, moreover, strange as it may seem, the only forms of the compound proposition which are ever treated by the logician, and so great is the apparent difficulty of these forms even, to one who has not been trained in Symbolic Logic, that a very recent writer of a text-book actually supposes that

Either *A* is *B* or *C* is *D*,

and

Either *A* is *B* or *C* is sometimes not *D*,

are propositions which are denials each of the other.

The actual denials of all four ways of saying one and the same thing are as follows (they are, of course, *particular* sequences) :

UNIVERSAL.	PARTICULAR.
Ǝ. Either some must dance or all must sing.	ə. 'T is not necessary that some should dance or else all sing.
A. If none dance, all must sing.	a. If none dance, not all need sing.
Ɐ. Only if some dance may some not sing.	ɐ. Not only when some dance may some not sing.
E. 'T is not permitted that none dance when some do not sing.	e. None may dance *and* some may not sing.

The *words* to be chosen are different according as the sequence is matter of logical or merely material following—in the one case we prefer, for instance, to begin A with *if*, in the other with *when*; but the *connection* between the two propositions is, in both cases, of the same formal kind, and subject to the same symbolical treatment. Of Mr. Mitchell's coupet of sequences,

Ǝ. Either no *a* is *b* or some *c* is *d*.
e. Sometimes some *a* is *b* and no *c* is *d*,
or, It may be that
some *a* is *b* *and* that no *c* is *d*,

it happens that the universal is one of the two most natural, and the particular is the very most natural of them all. That is not, of course, true of his simple propositions; "everything is either not a king else a tyrant" is the least frequently natural of all the forms of expression. It would be interesting to explain, if it could be done, why the relations of terms and the relations of propositions have fallen, in real life, into such different grooves as regards expression.

What I hope to have shown is that two systems of logic are not made the same system by the fact that both are systematic methods of procedure, nor yet by the fact that both express the common part and the aggregate of two terms in the same way; that two systems which have in every respect different rules of procedure, basedpon the fundamental tal differences of their several copulas, are, in any natural sense of the phrase, different systems; that there are sixteen different forms of symbolic logic possible, the combinations of any one of the four universal propositions with any one of the four particular; that eight of them (the combinations of a symmetrical with a non-symmetrical form) are wholly uninteresting—that is, they occasion confusion without securing naturalness; that, of the other eight, what suggests itself most naturally is the combination of each universal with the particular which immediately denies it, but that the combination with the *other* particular of the same symmetry has the extreme advantage, as a working method, of offering one single set of rules for dealing with particulars and universals; that no system of logic is complete which does not take account of particular propositions; that Mr. Mitchell's system of logic combines naturalness with facility of manipulation (for compound as well as simple propositions) in a higher degree than is possible to any other system; and, in the first place, that the lack of interest which logicians are content to feel in Symbolic Logic is unreasonable, and based upon a mistaken conception of its nature.

MEMORY, HISTORICALLY AND EXPERIMENTALLY CONSIDERED.

W. H. BURNHAM, PH. D.

IV.

I.—*Recent Theories.*

The tendency of recent studies in physiology and pathology has been to establish the importance of physiological processes in all acts of retention and reproduction. Such studies have shown that, whatever be the ultimate relation of mind to body, memory is dependent upon physical processes. A cerebral process of some kind is the physical concomitant of an idea, and the condition of the reproduction of the idea is the repetition of the original cerebral process. In some way the brain centers are modified by impressions; they retain in growth the form of their modifications; and, on occasion of appropriate stimuli, they tend to repeat processes that have once occurred. The following passage from a recent article by a French psychologist illustrates some of the facts that enforce this view: "It is evident that there is in memory something automatic, capable of functioning alone. Even the diseases and illusions to which it is subject prove that there is something delicate and fragile in this marvel of natural mechanism. If a scholar, after having received a violent blow on the head, forgets all his knowledge of Greek without forgetting anything else, and if afterwards, as the result of a second blow, he suddenly regains his lost Greek, it is difficult to see in memory, with M. Ravaisson, an act entirely spiritual. The automatic side of memory, especially of 'passive memory,' is illustrated by certain remarkable facts, where things have been preserved and reproduced with a facility that is at once perceived to be mechanical. When an imbecile in the asylum at Earlswood can repeat accurately a page of any book read years before and even without understanding it; when another person can repeat backwards what he reads as if he had under his eyes a 'photographic copy of the impressions received'; when Zukertort plays blindfolded twenty games of chess at once without considering anything but the imaginary chess-boards; when Gustave Doré or Horace Vernet, after attentively regarding their model, paint its portrait from memory; when another painter copies from memory Rubens' Martyrdom of St. Peter with an accuracy to deceive the experts, we conjecture that such accurate preservation and reproduction of the impressions received must have their causes in the organs."[1]

Some, however, among recent writers vigorously oppose any physical theory of memory. For example, Prof. Borden P. Bowne, the eminent American disciple of Lotze, maintains that "a physical explanation of memory cannot be found," and that "a mental explanation is equally impossible." Nevertheless, in some mysterious way the soul in its process of perpetual change "carries its past with it; not, however, in the form of latent modifications, but solely in the power of reproducing that past in consciousness."[2] Others, while emphasizing the importance of physical mechanism in retention and reproduction, maintain that the higher acts of conscious memory cannot be explained by physical processes. Thus Prof. Ladd, who holds that "the physical basis of memory as retentive is laid in the habit, or acquired tendency, of the elements of the nervous system,"[1] maintains that no physical conditions immediately concern the "mental activity which constitutes the essence of *conscious* memory."

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[1] From Fouillée's Le mechanisme de la mémoire, Revue des Deux Mondes, 15 Mai. 1885, pp. 359, 360.

[2] Metaph., p. 426. Ibid, p. 427. For a fuller account of Prof. Bowne's position, and a criticism of cerebral theories of reproduction, see Introduction to Psych. Theory, Ch. III, and Appendix to the same. Cf., also Ferri: La psychologie de l'association depuis Hobbes jusqu'à nos jours; and Huber: Das Gedächtniss.

"What is explained, if anything, is simply why I remember one thing rather than another — *granted the mind's power to remember at all.* This power is a spiritual activity wholly *sui generis,* and incapable of being conceived of as flowing out of any physical condition or mode of energy whatever."[2]

Fouillée also, after showing the dependence of memory upon physical processes, emphasizes the part that consciousness plays in a complete act of memory. He writes: "Not only consciousness, in its reflective form, has thus the power of reacting on the conservation and mechanical association of ideas, but also it is absolutely necessary to that third function which is the true characteristic of mental memory, the recognition of what is recalled. The automatism which we have described explains merely the revival of similar ideas and not their recognition as similar. . . . Memory is indivisibly physical and mental, physical for the spectator from without, mental for the spectator from within."[3]

Few if any will now be found to deny the dependence of memory in some degree upon cerebral processes. Even Prof. Bowne, with all his hostility to physiological theories of memory, admits that

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[1] See Elements of Phys. Psych., p. 552.
[2] Ibid, p. 556.
[3] Loc. cit., pp. 388, 389.

"the brain conditions the mental activities of thought and recollection."[1] The ultimate nature of memory, especially in its function of recognition, being an inscrutable mystery, its physical side then appears the promising field for study. At all events most of the valuable contributions to the psychology of memory in recent years have been made by students in this field. Some of the theories that have resulted from their studies may be briefly mentioned here.

Accepting then the theory that memory — at least as retentive and reproductive — has a physiological basis, what is the physical mechanism that makes the persistence of an impression possible? This is a question that in recent years has divided physiologists. Three theories have been advocated. It is supposed (1) that memory depends upon a *movement* persisting in the brain; (2) that it depends upon a persisting *trace* or *residuum* in the brain; (3) that it depends upon a *disposition* persisting in the brain.

The theory that memory depends upon persisting movement has recently been advocated by Mr. Luys.[2] He calls that property of the nervous elements by virtue of which vibrations in them persist *phosphorescence*, since it seems analogous to that property of phosphorescent substances which causes them to remain luminous after the source of their light has disappeared. Luminous vibrations, as shown by the investigations of Niepce de Saint Victor, may be garnered up in a sheet of paper and by the use of proper re-agents, made to appear months afterward. The same persistence of vibrations is illustrated in the common forms of photography.

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[1] Introduction to Psychological Theory, p. 113.
[2] The Brain and its Functions, Book II.

A plate of dry collodion retains for weeks the changes produced in it by exposure for a few seconds to the sun's rays. Similarly in memory, vibrations of an impression persist in the nervous elements, and to revive the impression it is only necessary that a suitable awakening agency renew the vibrations.[1]

The second theory, that memory depends upon a persisting trace, has long been a favorite one. Plato taught it in his figurative way. And among the moderns it has not lacked adherents since the days of the physiologist Haller. Among recent writers Oliver Wendell Holmes has eloquently illustrated it.[2] He deems the hypothesis probable, "that memory is a material record; that the brain is scarred and seamed with infinitesimal hieroglyphics, as the features are engraved with the traces of thought and passion."[3] A favorite illustration of this view has been the comparison of the brain in memory to a phonograph.[4] Like the tinfoil of the latter, the brain preserves a trace of impressions made upon it, and may be called a "conscious phonograph."[5]

Richet has advocated a form of the same theory,[6] and maintained that there is a great difference between the muscles and the nerves, since the effect of stimulation upon the former is transient, while upon the latter it is permanent. "In a word," he says, "while the muscles and the organic nerve-cell return completely to their original condition after an excitation, the psychic nerve-cell does not. It has been modified in a permanent manner by the act of stimulation; and this modification can be effaced only by the death of the cell. Each excitation has, so to speak, created a new cell different from the first." [1]

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[1] This theory merges into the second; and some writers do not clearly state which is their own view. Draper's illustrations may be taken in connection with either theory. See Conflict between Science and Religion, p. 132, seq.; also, Human Physiology, ch. xxi. The examples from photography also are often used to illustrate the second theory.

[2] Cf. Mechanism in Thought and Morals, p. 69 seq.

[3] Ibid., p. 77.

[4] See The Secret of a Good Memory by Dr. Mortimer-Granville; also an article by M. Guyau, La Mémoire et le Phonographe, Revue Philosophique, March, 1880.

[5] M. Guyau, loc. cit., p. 322.

[6] Les origines et les modalités de la mémoire, Revue Philosophique, June, 1886.

The third view — that the essence of memory is a functional disposition persisting in the brain — is, perhaps, the one most widely held by contemporary psychologists. Among recent writers Wundt[2] and Ribot[3] have been the most prominent exponents of it. But it is not new. Descartes taught it in a dim way.[4] Malebranche[5] and Bonnet[6] held substantially the same view, except that they thought of the disposition as persisting in the nerve fibres, while modern physiologists attribute the storing up of impressions especially to the nerve cells. Hartley's theory of vibratiuncules reminds one of Aristotle and seems to belong in our first class; but he also speaks of the persisting dispositions to these diminutive vibrations;[7] and some of his disciples appear to have held the same view. The theory has already been mentioned, but it may be summed up as follows: When a nerve cell, or a group of cells, receives an impression and reacts, it is modified in some way. There is, perhaps, a change in the molecular structure of the cell—of what sort we do not know. It involves, however, an aptitude or disposition for reacting again in the same way. Just as the eye that is constantly practiced in comparing and measuring acquires an aptitude or skill that makes it far superior to the untrained eye. This is a functional, not an organic, change in the eye. In like manner every state of consciousness affects the nervous elements of the brain, and the nerve center is functionally modified so that the recurrence of the same mental state is rendered easier.

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[1] Ibid,, p. 565.

[2] See his Grundzüge d. phys. Psych., Bd. II, ch. 17, p. 306 seq. 2 te Aufl.; also my 2d article, p. 261.

[3] See Diseases of Memory, Ch. I.

[4] Cf. my 1st article, p. 66.

[5] Cf. Recherche de la Vérité, II, v, 3; also my 1st article, p. 67.

[6] Cf. Analyse abrégée, ¶ X; also my 1st article, p. 79.

[7] See my 1st article, p. 82.

Ribot lays stress also upon the establishment of dynamic associations between the nerve elements.[1] The basis of memory, in his opinion, consists not only in the modifications effected in the individual elements, but also in the way various elements are grouped together to form a complex. In so simple a matter as the recollection of an apple, for example, these associations are necessary; for the recollection of an apple is the repetition in weaker form of the perception of the apple. This perception involves not only the retina and transmission along an extended route to the cortex, but association with the motor centers that control the movements of the eye; for it is owing to the latter that we perceive the apple as a solid body. The process of education renders stable these dynamic associations; and "a rich and well-stored memory is not a collection of impressions, but an assemblage of dynamic associations, very stable and very readily called forth."

These three theories, as held by recent writers, are closely related and merge in one another. The movement and the trace may cause the functional disposition. The functional disposition itself may, in the last resort, be due to persisting movement. At present it is impossible to say just what does occur. But the third theory, allying as it does the mechanism of memory to well-known physiological facts, and providing for the retention in growth and transmission to new cells, by a kind of heredity, of these acquired tendencies, presents fewest difficulties, and explains the facts perhaps as well as any.

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[1] Loc. cit. Cp. also Wundt, loc. cit. These associations depend upon the general law, that when a nervous discharge has occurred along a certain path, discharge along the same path is rendered easier.

In the second place, as has been often shown by recent writers, studies in physiology and pathology indicate that we have not memory, but memories. Each sense and organ of the body may be said to have its memory; and these memories are in a measure independent of each other. The visual memory may be lost without affecting the auditory memory. The memory of speech may be impaired and the general motor memories remain intact; and so on. Loss of any of these memories is due to disease in the appropriate center in the brain. Thus it appears that instead of one seat of memory as the older psychologists supposed, the different areas of the brain not only have their special office to perform in governing the organs of the body, but they are also the seat of the respective memories of those organs.[1]

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[1] While many of the doctrines of localization are still disputed, few physiologists will deny that our various memories are scattered over the brain, the percipient organ, whatever it is, being also the remembering organ. Apart from physiology and the consideration just mentioned, the view has also been held quite frequently that we have memories rather than a faculty of universal memory. Cardinal Newman writes: "In real fact memory, as a talent, is not one indivisible faculty, but a power of retaining and recalling the past in this or that department of our experience, not in any whatever. Two memories, which are both specially retentive, may also be incommensurate. . . . . There are a hundred memories as there are a hundred virtues." Grammar of Assent, Am. ed., New York, 1870, pp. 327, 328.

The cases where men and animals have lost certain memories and then recovered them may sometimes be explained according to the hypotheses of Wundt and others, as follows: Around that area of the brain which functions any sense or movement is an area that might assume the same office. It is a *potential* area for the memories of that center. For when the actual center of any sense is destroyed a part of this potential area may be educated, so to speak, to perform the necessary functions. For example, if the actual sight area in the posterior part of a dog's brain be cut out, and the potential area remains, the dog becomes blind, but may recover. If, however, the potential area also be destroyed, the dog remains permanently blind.[1]

From what has been said the contrast will be clear between the old view, which made memory a distinct faculty and located it in a definite part of the brain and the modern doctrine of physiology which tends to consider memory as a function of all the cerebral centers.

The next problem that physiology has attempted to solve is in regard to the physical mechanism of the association of ideas. Psychologists from Aristotle down have generally been content with stating the phenomena of association. Modern physiological psychology asks: "Why do ideas recall each other according to the laws of contiguity, similarity, and contrast?"

Fouillée, following Herbert Spencer, has attempted to answer this question. In his opinion the two laws of similarity and contiguity[2] are, as far at least as the physical side is concerned, two expressions of the same law. When two ideas are associated by contiguity, the physical mechanism is this: Two nerve-currents are aroused in neighboring parts of the brain. They are diffused according to the law of the persistence of movement until they meet and commingle. This union is the germ of habit or unconscious memory. It can, of course, take place only in case of nerve-currents in contiguous parts of the brain. The mechanism for association of similar ideas is the same; for adjacent parts of the brain are similar. Thus Fouillée agrees with Spencer that the prime law of association is this: "*Every presentation tends to associate with similar presentations on account of the identity of their seat in the brain.*"[1] All other laws are subordinate.

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[1] Cf. an excellent article by Dr. M. Allen Starr, "Where and How We Remember," Pop. Sci. Monthly, Sept., 1884. Some, however, maintain that the sight-center is not located entirely in the cortex.

[2] Contrast may be omitted as it is a subordinate principle.

"Contiguity in time then," he says, "links things only by means of a contiguity in the extension of the brain. Thus are established in the nerve paths, as on the railroads, junctions, analogous to those where the switchman determines the course of the trains. The succession of ideas, even of those which we recognize afterwards as similar, is caused by the meeting, at the point of bifurcation, of two trains of images in contiguous parts of the brain. The words *entremets*, *entrecote*, *entrepont*, will mutually arouse each other by their point of bifurcation *entre*, and in some diseases the patient repeats these words mechanically one after the other." The cementing power is the persistence of force and the continuance of movement which communicates itself always to adjacent parts. All movement expends itself in one way or another. It cannot stop then, in a group of brain cells, but necessarily passes to neighboring groups, and persists even to the most distant groups through the connection of the association fibres.

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[1] Loc. cit., p. 381.

Fouillée criticises Spencer, however, for confusing the mechanical process of joining similar images with the consciousness of their similarity. The latter is a mental synthesis. It occurs only between impressions presented automatically, but should be distinguished from the mechanical process of presenting such impressions. The reacting and synthesizing work of consciousness should be noted, especially in the complicated operation of recognizing the similarity between the past and the present. His position is summed up in the following passage:

"There is no contiguity, in our opinion, without similarity. Objectively, contiguity itself is a kind of similarity, under the relation of space and time; for it is a meeting in the same space and the same time which always tends to a certain fusion of movements the most opposite into a common form of movement. Subjectively, contiguity always becomes a certain similarity in consciousness. The mere fact of perceiving that disparate things *coincide*, as a bright light, a sound, a pain, is already a consciousness of similarity in the very bosom of difference. This judgment involves a reaction of consciousness in respect to sensations coming to it; and it is this reaction which constitutes the mental synthesis. This synthesis, without doubt, can occur only between terms already given by pure automatism, but consciousness perfects and renders permanent the cementing begun by simple mechanical coincidence. . . . . The brain takes note only of contiguity, of which similarity is a consequence; intelligence notes only similarity, of which contiguity is for it a simple kind and outline."[1]

Thus consciousness, according to Fouillée plays an important rôle in the association of ideas. It reacts on presentations and arranges them harmoniously. While not the primitive force in association, it becomes, by reacting on associations presented to it, the force that dis-associates and analyzes. It has not only the negative power of rejecting ideas, but also the power of increasing by reflection the force of ideas suited to its purposes, thus making one idea a center of attraction, as it were, for other ideas.

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[1] Loc. cit., pp. 382, 383.

In criticism of Fouillée's explanation of the physical mechanism of association, it may be said that it is too simple for an adequate explanation of the incalculable complexity of thought. The kaleidoscopic train of our ideas follows no such simple rules as the traditional laws of association. Fouillée himself, as well as Horwicz and others, has shown the part that feeling plays in the recall of ideas. Hodgson has done the same in showing that association by interest should be added to the other laws, and James gives cogent reason for adding a principle of spontaneous revival.

The importance of the last two principles will be seen by a moment's reflection. Not only does our mental constitution demand an adaptation on the part of ideas to the emotional mood of the moment, and thus our thoughts take a different course as we are sad or happy; but we find interest constantly shunting our thoughts into new channels. This can be readily shown by an illustration similar to those given by Hodgson and James. Suppose I am repeating the oft-quoted lines of Shakespere:

> "Who steals my purse, steals trash," etc.

If none of the words suggest to my mind an idea of peculiar interest, I run through the passage without distraction. But suppose my pocket-book happens to be unusually empty and I know of no easy means of replenishing it. Then, when I come to the word *purse*, I am likely to hesitate, and my thoughts will probably shoot off upon my financial condition, because that interests me more than Iago's reflections upon the value of a good name and the baseness of calumny.

In this case contiguity works with interest, but contiguity alone would carry my thoughts on without interruption to the end of the passage.

As an example of the spontaneous revival of images, Prof. James mentions the phenomenon sometimes observed before going to sleep when images dance in haphazard fashion before the eyes. This phenomenon occurs most frequently, perhaps, when we are weary; and it may be due, in part, to peripheral excitation. A case of more clearly mental revival seems to occur when we are well rested, and the nerve cells are, perhaps, overcharged with energy. At such a time it often happens that a multitude of images seem to spring up in the background of consciousness and to crowd around our conscious train. Our thought, to use a phrase of Fouillée's, seems to be a process of "intelligent selection." Of course it may be argued that, even in the case of this semi-conscious revival, there are subtle links of subconscious association, but there seems to be an element of spontaneity here, that is lost sight of in the ordinary doctrine of association.

Again the revival of our ideas is determined by the law of habit.[1] Modern studies add weight to the dictum of Bacon that "custom is the principal magistrate of man's life"; for they seem to show that our very thoughts chase each other along habitual paths. Except perhaps with men of genius, it is only in rare moments of special inspiration that our thoughts flash in unwonted channels.[1]

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[1] The relation of memory to habit has long been recognized. For a recent theory explaining memory by habit see Gratacap, Théorie de la Mémoire.

Hodgson has shown how habit works with interest in the spontaneous recall of our ideas.[2] "Two processes," he says, "are constantly going on in redintegration, the one a process of corrosion, melting, decay, and the other a process of renewing, arising, becoming. Unless by an effort of volition, which is here out of the question, (since here only the spontaneous train of thought is under consideration,) no object of representation remains long before consciousness in the same state, but fades, decays, and becomes indistinct. Those parts of the object, however, which possess an interest, that is, those which are attended by a representation of pleasure or pain, resist this tendency to gradual decay of the whole object. . . . . . . This inequality in the object, some parts, the uninteresting, submitting to decay, others, the interesting, resisting it, when it has continued for a certain time, ends in becoming a new object." This new object consists of the interesting parts of the old object and some sort of an indistinct environment. It is formed under the general law of redintegration, which is, as formulated by Hodgson, "that every object, which has occurred in a variety of combinations, has a tendency to redintegrate, or call back into consciousness, all of them."[3] These interesting parts which persist in consciousness then, as soon as the uninteresting parts of the old object have vanished, are free to unite with any object with which they have at any time been combined before. "All the former combinations of these parts may come back into consciousness; one must; but which will? There can be but one answer: That which has been most habitually combined with them before. This new object begins at once to form itself in consciousness, and to group its parts round the part still remaining from the former object; part after part comes out and arranges itself in its old position; but scarcely has the process begun, when the original law of interest begins to operate on this new formation, seizes on the interesting parts and impresses them on the attention to the exclusion of the rest, and the whole process is repeated again with endless variety."[1]

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[1] The pathological activities of the mind are instructive upon this point. Paranoia is a case of extreme habituation. The patient has a bad habit of thought. For an excellent illustration of the way the thought of the paranoiac gravitates into one channel see a paper on Insistent and Fixed Ideas, by Edward Cowles, M. D. AM. JOUR. PSYCH. I, 222.

[2] Space and Time, p. 266 seq. Cf. also, Theory of Practice, vol. I, p. 367–381.

[3] Space and Time, p. 267.

Such is the ceaseless interplay of habit and interest in our spontaneous trains of thought, and our voluntary thought seems still more complicated. It is not necessary to discuss the latter here; but it may be noted that in our ordinary trains of thought the spontaneous and the voluntary alternate, and thus the complexity is still more increased.

In the return of ideas to consciousness then, we find the laws of association modified by other principles, such as congruity with the emotional mood of the moment, habit, interest, spontaneity; and, in addition of course, by the recency and the vividness of the first impression. These are the factors that from the subjective standpoint seem to determine the train of thought in recollection, and it is natural to suppose that the corresponding physical processes are equally complex. The theory of Fouillée can afford but a partial explanation of them; all that can be done seems to be to express in the most general terms the cerebral activity upon which recollection depends.

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[1] Ibid., pp. 267, 268.

Prof. James has attempted to do this by showing how the laws of association "may follow from certain variations in a fundamental process of activity in the brain."[1] Basing his theory upon the principle that, when two brain processes have occurred simultaneously or in immediate succession, either of them on recurring tends to arouse the other also, and noting the complexity that arises from the fact that each elementary tract has at different times been excited in conjunction with *many* other tracts, and in view of the tension in nerve-tissue and the summation of excitements, he formulates the law of association from the physical standpoint as follows: "*The amount of activity at any given point in the brain-cortex is the sum of the tendencies of all other points to discharge into it, such tendencies being proportionate* (1) *to the number of times the excitement of each other point may have co-existed with that of the point in question;* (2) *to the intensity of such excitements; and* (3) *to the absence of any rival locality or process functionally disconnected with the first point, into which the discharges might be diverted.*"[2]

Another important work of modern psychology has been to show the relation of memory to the time sense. The metaphysical doctrine of the present as a knife-edge has no foundation in experience. Such a present, the mere conterminous of the past and future, may be a metaphysical reality, but it is a psychological nonentity. The only present of which we practically know anything is the immediately intuited past. "If the present thought," says Prof. James, "is of A B C D E F G, the next one will be of B C D E F G H, and the one after that of C D E F G H I,—the lingerings of the past dropping successively away, and the incomings of the future making up the loss. These lingerings of old objects, these incomings of new, are the germs of memory and expectation, the retrospective and the prospective sense of time. They give that continuity to consciousness, without which it could not be called a stream."[1]

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[1] See The Association of Ideas, Pop. Sci. Monthly, March, 1880.
[2] Ibid., p. 582.

The few seconds during which objects are fading from consciousness has been aptly named the "specious present" by Mr. Clay and Prof. James. The most vivid part of it is measured by the experiments on the *Umfang*, or extent of immediate consciousness for successive impressions.[2] Under the most favorable circumstances it may amount to some dozen seconds,[3] with perhaps an obscure background extending still farther into the past. It forms not only "the original paragon and prototype of all conceived times," by reference to which, the past and the future are measured, and without which we should probably have no sense of time, but its echoes of sensations just past make Fechner's memory-after-images possible[4] and constitute what Exner calls our "primary memory,"[5] and Richet our "elementary memory."[6]

That a stimulus leaves behind it an excitation that only gradually passes away is proved first by such facts as the possibility of perceiving an electric spark that lasts only a hundred-thousandth of a second,[7] the well-known fact of the summation of stimuli, and the phenomena of sensorial after-images; secondly, by the vividness of the memory-after-image, which renders possible an accurate comparison of impressions. To quote again from Prof. James's admirable paper: "We may read off peculiarities in an after-image left by an object on the eye which we failed to note in the original. We may 'hark back' and take in the meaning of a sound several seconds after it has ceased. . . . . With the feeling of the present thing there must at all times mingle the fading echo of all those other things which the previous few seconds have supplied. Or, to state it in neural terms, there is at every moment a cumulation of brain processes overlapping each other, of which the fainter ones are the dying phases of processes which, but shortly previous, were active in a maximal degree. *The amount of the overlapping determines the feeling of the duration occupied.*"[1]

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[1] Wm. James: The Perception of Time, Journal of Speculative Philosophy, XX, pp. 375, 376.

[2] See Wundt: op. cit., Bd. II, p. 213 seq.

[3] Cf. Dietze's experiments, Philos. Studien, II, p. 362.

[4] See my 2d article, AM. JOUR. OF PSYCH., II, 253, 254.

[5] See Hermann's Handbuch d. Physiol., Bd. II, Thl. II, p. 281.

[6] Loc. cit., p. 562.

[7] Cf. Richet, loc. cit., pp. 561, 562.

Many of the older writers were wont to say: "Our notion of duration, as well as our belief in it, is got by the faculty of memory."[2] This is true in a certain sense. Memory and the feeling of duration are inseparably connected; both alike seem to depend on the decaying processes — not only sensations but acts of attention and of thought as well — that fill the immediately intuited past. All that is necessary for the elementary form of either is the simultaneous presence of an active and a fading image. If a man were instantaneously created "with a brain in which were processes just like the 'fading' ones of an ordinary brain, the first real stimulus after creation would set up a process additional to these. The processes would overlap;

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[1] Loc. cit., p. 401.

[2] Reid, Essays on the Intellectual Powers of man, Essay III.

and the new-created man would unquestionably have the feeling, at the very primal instant of his life, of having been in existence already some little space of time."[1]

The mysteries of memory and the sense of time are by no means solved, but the discussion has shifted from metaphysical to psycho-physic ground; and they who would study memory and the feeling of duration in their elementary form, must study the *Umfang* of consciousness.

Finally the feeling of personality depends upon memory. Or rather, with John Stuart Mill, we may say: "The phenomenon of Self and that of memory, are two sides of the same fact, or two different modes of viewing the same fact. We may, as psychologists, set out from either of them, and refer the other to it. We may in treating of memory, say that it is the idea of a past sensation associated with the idea of myself as having it. Or we may say, in treating of Identity, that the meaning of Self is the memory of certain past sensations."[2] Neither of these facts can be explained; but probably both alike depend primarily upon the persistence of sensations and the gradual fading of images. But neither would be complete without the revival of impressions. With the present perceptions mingle the memories of similar perceptions. If the object before us is a tree, the memories of other trees arise along with the sensations from it. With the impressions A, B, C, D, come the memories a_1, a_2, a_3, b_1, b_2, b_3, and so on. "Our consciousness is then," as Richet says, "always in the presence of a certain limited number of old images, that are always nearly the same, and these images being related to the same *me*, are the basis of the individual's personality — a personality that is rendered stable enough by the community of the images."[1]

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[1] James, loc. cit., p. 406.

[2] Note in his edition of James Mill's Analysis, Vol. II, p. 174.

Recent pathological studies have furnished some remarkable evidence for the view that memory and the feeling of personality are "two sides of the same fact." Whenever in disease, or somnambulism, or by hypnotic experimentation the old memory-images are shut out of consciousness, a new personality results.[2] If the conscious life is broken up into several memories, as many new personalities are improvised. It is not necessary to recount here the well-known hypnotic experiments at Paris, Nancy, and elsewhere; nor does space permit a discussion of the question whether the results of such studies affect the old notion of a transcendental self; but it is clear that the integrity of the personality depends upon the unity of memory, and anything that seriously affects the latter is likely to affect the former also.

II.—*Experimental Studies.*

The most important attempt to apply the methods of experimental psychology to the study of memory was made a few years ago by Dr. Ebbinghaus of Berlin. What he modestly calls the provisional results of his investigation are reported in a valuable monograph.[3] He experimented only upon himself; and, as he admits, his results have directly only an individual value. Nevertheless, his long and laborious series of experiments are so valuable both for their immediate results and as showing the advantages and limitations of psycho-physic methods in the study of memory, that a brief abstract of his work will be given here.

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[1] Loc. cit., p. 589.

[2] For examples cf. Ribot: Maladies de la personnalité.

[3] Ueber das Gedächtnis, Leipzig, 1885.

As the basis of his study Ebbinghaus lays down the principle that psychic states of every kind are not annihilated when they vanish from consciousness, but their effects persist unconsciously. Three classes of facts prove the persistence of states of consciousness apparently lost: 1st. in many cases we can *voluntarily reproduce* the lost states; 2d. what has once been present to consciousness may return, sometimes after years, without any special effort, i. e., is *involuntarily reproduced*; 3d. vanished states of consciousness give evidence of their continued existence by rendering similar thought processes easier.

Great difficulties are encountered in experimenting upon memory. The conditions upon which a good memory depends vary with different individuals, and with the same individual from morning to evening, from youth to age. Moreover, much depends upon the content of the matter to be remembered. It is hard, for example, to remember forms and colors; and past states of feeling can be reproduced with difficulty. In the first place then, how shall the multitude of determining conditions be kept constant? In the second place, what numerical measure shall be found for anything so complex and evanescent as psychic processes? Ebbinghaus attempted to overcome these difficulties as follows.

From the simple consonants and the eleven German vowels and diphthongs all the syllables possible of a definite kind were formed, placing a vowel between two consonants. These nonsense syllables, about 2300 in number, were shuffled, and then, as they were drawn by lot, formed into series of different lengths. Several of these series were used at each experiment. The syllables were read aloud repeatedly until it was just possible to recall them voluntarily. This end was considered attained "when a series, the first syllable being given, could be repeated by heart for the first time without hesitation, at a given rate of speed, and with the consciousness of perfectness."

This nonsense material has the following advantages. In the first place, it is relatively simple and homogeneous. While prose or poetry constantly changes in character, and varies in interest with the individual apperception, among more than 2,000 of these syllables, scarcely a dozen make any sense, and in the process of learning, the thought of this meaning is seldom aroused. The difference, however, between sense and nonsense syllables is not as great in this respect as would be expected *a priori*. For the varying content of the former is offset in case of the latter by predilections for different letters and syllable-combinations due to the influence of the mother-tongue. The advantage of nonsense material is considerable, however, in two other respects, namely, that it affords an inexhaustible amount of new combinations of similar character, and that it offers means for convenient quantitative variation.

In learning these nonsense syllables careful attention was given to details of method. A series was read through completely from beginning to end; then the first syllable was read, and the attempt was made to repeat the series. At the first hesitation the remainder was read, and then the repetition was begun anew. Reading and repetition were as nearly as possible at a uniform rate, namely, 150 syllables a minute, regulated by the ticking of a watch. The voice was kept at a uniform pitch with the accent upon every third or fourth syllable. After learning a series a pause of 15 seconds was made to note results, and then the next series was attempted. Disturbances were removed, the attention concentrated, and the aim was to learn the series as quickly as possible. No attempt was made to bind the syllables by fancied relations. And, finally, care was taken especially that all the conditions of life during the period of experimentation should be as nearly uniform as possible. In the case of any great change in the mode of life, Ebbinghaus deferred the experiments, and a period of practice preceded their renewal. Similar experiments were performed at the same hour of the day, and care was taken that the activity preceding the experiments should be of the same character.

Two sources of error are noted by Ebbinghaus. First there are flickerings of the attention, or the like, that cause variations in the ability to repeat a series perfectly. These variations, however, may balance in a large group of series. The other possible source of error is a very dangerous one. It is the unconscious influence of theories and opinions of the experimenter. Even if he tries to avoid prejudice, that very effort is liable to vitiate the results. But, as the aim of the experiments was to obtain relative rather than absolute results, Ebbinghaus concluded that there was no reason to be very suspicious on this account.

The experiments were made at two periods, namely: 1879–80, and 1883–84, each period extending over a full year. Testing experiments of a similar kind preceded each period, so that in the results given the effect of growing practice may be disregarded. In the later period all the experiments were made between the hours of 1 and 3 p. m.; in the earlier one, they were divided unequally into three groups, performed between 10 and 11 a. m., 11 and 12 a. m., and 6 and 8 p. m.[1] By taking groups of an equal number of successive series separated by considerable intervals of time, the variations due to fatigue, fluctuation of the attention, and the like, were largely balanced. This detailed account of the method is necessary to a just estimation of the results to be detailed.

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[1] The latter groups will be designated A, B, and C respectively.

The first point is the dependence of the rapidity of learning a series upon the length of the same. It is well known that it takes a proportionately longer time to acquire a long series of ideas than a short series. Ebbinghaus's experiments corroborate this. He found that he could generally repeat a series of seven syllables after once reading them. Sometimes eight syllables could be repeated perfectly after one reading, six almost always; but about seventeen repetitions were required for a series of twelve, and nearly thirty for one of sixteen syllables. Of course these results have only an individual value; but it is noteworthy that for this experimenter they long remained constant.

By another group of experiments the process of learning poetry was compared with that of learning nonsense syllables. Stanzas from Byron's Don Juan were learned, and Ebbinghaus estimated that only about one-tenth the number of repetitions was required for learning material thus connected by the bonds of sense and rhythm.

The next important problem was the relation of retention to the number of repetitions. When a series once learned is forgotten, the strength of the impression persisting unconsciously can always be measured indirectly by the saving of time required for re-learning the series. To determine the effect of many repetitions upon the readiness with which a series can be reproduced, Ebbinghaus made seventy double experiments of which each experiment consisted in reading or repeating six sixteen-syllable series a definite number of times, and then, twenty-four hours later, re-learning them. The experiments were divided into seven groups of ten experiments each, the number of repetitions in the seven groups being respectively 8, 16, 24, 32, 42, 53 and 64. The results are embodied in the following table,[1] the numbers being the number of seconds required for the re-learning.

	After 8 repetitions.	After 16 repetitions.	After 24 repetitions.	After 32 repetitions.	After 42 repetitions.	After 53 repetitions.	After 64 repetitions.
	1171	998	1013	736	708	615	530
	1070	795	853	764	579	579	483
	1204	936	854	863	734	601	499
	1180	1124	908	850	660	561	464
	1246	1168	1004	892	738	618	412
	1113	1160	1068	868	713	582	419
	1283	1189	979	913	649	572	417
	1141	1186	966	858	634	516	397
	1127	1164	1076	914	788	550	391
	1139	1059	1033	975	763	660	524
Mean.	1167	1078	975	863	697	585	454
Probable error.	±14	±28	±17	±15	±14	±9	±11

The average time for the first learning of six series of sixteen syllables each, as estimated from fifty-three experiments was 1270 seconds, with the small probable error of ± 7. Comparing with this the times required for re-learning as given in the table, Ebbinghaus estimates that, on an average, in re-learning there is a saving of one third due to the persisting after-effect of the repetitions. In other words for every three repetitions to-day a person is saved about one repetition twenty-four hours afterwards.

The same experiments show the relation of recognition to the number of repetitions. When the series were impressed upon the mind with only eight or sixteen repetitions they were not recognized on the following day. When, however, there were fifty-three or sixty-four repetitions, they generally were recognized.

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[1] Op. cit. p. 75.

Further experiments indicated that this saving does not continue in the same ratio when the number of repetitions is increased above sixty-four. If for each repetition one third its value is saved in learning the series the following day, then it would seem that, if a series is repeated three times as much as is necessary to learn it, it should be possible to recall the series the next day. But even with four times the number of repetitions, this proved impossible.

A still more important group of experiments showed the relation of retention to time. 163 double experiments were performed. Each experiment consisted in learning and re-learning eight thirteen-syllable series. The re-learning was done at seven different intervals, approximately as follows: one-third of an hour, one hour, nine hours, one day, two days, six days, and thirty-one days. The learning was continued until it was possible to repeat a series twice without error. The time was measured from the completion of the learning. The results appear in the following table.

Time interval.	No. of seconds required for first learning.	No. of seconds required for re-learning.	Gain in seconds.	Gain in per cent.	Probable error.
19 min.	1081	498	583	58.2	1
63 “	1106	647*k*	459	44.2	1
525 “	1132	752*k*	380	35.8	1
1 day A	1109	756	353	33.8	2
“ B	853	599	254	32.6	2.2
“ C	1184	803	381	34.6	2.3
2 days A	1154	854	300	27.2	2.3
“ B	891	647	244	28.2	3.5
“ C	1245	917	328	28.1	1.8
6 days A	1090	834	260	25.2	1.9
“ B	872	652	220	26.1	4
“ C	1306	989	317	24.9	1.6
31 days A	1115	892	223	21.2	1.3
“ B	879	710	169	20.8	1.4
“ C	1261	1007	254	21.1	2.7

Examination of the table[1] shows that the process of forgetting is rapid at first and then slower. After the interval of an hour so much has been forgotten that more than half the original work must be done again before the series can be reproduced. After eight hours almost two thirds of the original work is necessary. But from that point the process of forgetting proceeds more slowly. After twenty-four hours the impression still retains about a third of its original strength; after six days, a quarter; after a month, still a full fifth. It is noteworthy that, while the impression made by nonsense syllables is so evanescent that a series once perfectly learned is forgotten after an interval of twenty minutes, a residuum of some sort persists for a long time, so that even after a month the same series can be re-learned in four fifths of the time originally required. A general statement of the results is as follows: The ratio of what is retained to what is forgotten is inversely as the logarithm of the time[1].

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[1] Op. cit. p. 94 seq. Ebbinghaus gives tables for each group of experiments. I have collected in this table the *mean* results only.

A, B and C designate experiments performed at the hours of 10—11 A. M., 11—12 A. M. and 6—8 P. M. respectively. In estimating the per cent. of saving, the time for the final two perfect repetitions, i. e., 85 sec., was deducted from the time for learning as given above. The figures followed by *k* in the 2d and 3d groups give the corrected times for re-learning. As there was an unavoidable variation in the conditions due to the increased fatigue at the time of re-learning in the later hours of the day, a number of seconds were deducted from the original results.

Another group of experiments showed the interesting result, that a long series is more strongly impressed upon the mind by once learning than a short series. For example, a series of thirty-six syllables is nearly twice as firmly impressed as one of twelve syllables.

Further experiments showed that when there are a large number of repetitions a suitable distribution of them over a certain interval of time is more advantageous than the accumulation of them all at once. Thus in learning nine twelve-syllable series the same impression was made by thirty-eight repetitions distributed over four successive days as by sixty-eight consecutive repetitions.

Ebbinghaus experimented also upon the association of ideas by contiguity. This law of association is generally formulated as follows: Presentations once aroused in consciousness simultaneously, or in immediate succession, reproduce each other, and more easily in the direction of the original succession, and with greater certainty the oftener they have been together. But one point may be mooted. When a series *a b c d e f g* has been learned, does *a*

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[1] Ebbinghaus's formula was:

$$\frac{\textit{das Behaltene}}{\textit{das Vergessene}} = \frac{K}{(\log.\ t)^c},$$

in which K and c are individual constants.

recall *b*, and *b* recall *c*, and so on, each member of the series being connected only with the succeeding member; or does *a* recall *b*, and tend also to recall *c*, *d*, and the rest, each member being associated not only with the succeeding member but also, though in a less degree, with the whole series? The Herbartians maintain that the latter is true for so many members of the series as have once been together above the threshold of consciousness.[1] To bring this problem to a scientific test, Ebbinghaus performed the group of experiments next to be described.

When a series of nonsense syllables is learned by heart, and re-learned in the same order on the next day, there is, as we have seen, a saving of about one third the original work. This one third measures the strength of the association from one member of the series to the succeeding member. Suppose the syllables are re-learned in a different order; what will be the result? Fifty-five double experiments were performed to determine this point. Six sixteen-syllable series were employed in each experiment. If the different series are represented by Roman, the syllables by Arabic numerals, the series employed would be as follows: I_1 I_2 I_3 . . . . . . I_{15} I_{16}, II_1 II_2 II_3 . . . . . . II_{15} II_{16}, and so on. New series were formed by Ebbinghaus from the alternate syllables as follows: I_1 I_3 I_5 . . . . . . I_{15} I_2 I_4 I_6 . . . . . . I_{16}. Except in the middle, where there is a break, each syllable of the transformed series was separated from its present neighbor by an intervening member in the original order. If these intervening members were essential hindrances to the bonds of association, then the new series is as good as unknown; and one can expect no saving of labor in learning the transformed series. If, on the contrary, the bonds of association extend not only from one member to the succeeding members, but also over intervening members to more distant syllables, then there will be a certain predisposition to the new series. The succeeding syllables of the series will still be united to each other with a certain strength, and less work should be required to re-learn them than to learn an entirely new series. The amount of this saving will be a measure for the strength of association over intervening members. If series are derived by skipping two, three or more syllables, the same considerations apply to them. Ebbinghaus derived similar series by skipping two, three, and seven syllables. Skipping seven, the derived group of series was as follows: I_1 I_9 II_1 II_9 III_1 III_9 IV_1 IV_9 V_1 V_9 VI_1 VI_9, I_2 I_{10} II_2 II_{10}, and so on, the last series being V_7 V_{15} VI_7 VI_{15} I_8 I_{16} II_8 II_{16} III_8 III_{16} IV_8 IV_{16} V_8 V_{16} VI_8 VI_{16}. By examining series derived in this manner, it will be seen that the breaks increase with the increasing number of syllables skipped. Where the different series are mixed, there is no association except between syllables that originally were in the same series. Hence, the derived series suffer from a certain unavoidable inequality.

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[1] See Herbart's Lehrb. z. Psychol., §29; also my 2d article, AMERICAN JOURNAL of PSYCHOLOGY, II, 248.

Each experiment consisted in learning the six original series; and then after twenty-four hours re-learning them in the derived order. The result showed a remarkable saving of time in re-learning the series. Skipping one, two, three, and seven syllables, the mean saving in learning the derived series was 152 sec., 94 sec., 78 sec., and 42 sec., the mean time for learning the original series being 1266 sec.

The objection may be raised that the syllables are impressed upon the mind by the first learning, not only in their definite order, but also as separate members. With reference to this objection, Ebbinghaus experimented, retaining the initial and final syllables of the six original series employed in an experiment, and using chance combinations of the other eighty-four syllables. In the case of series formed thus by mere permutation, the mean saving was only 12 sec., the mean time for learning the original series being 1261 sec.

Again, it may be objected that the saving of time in re-learning was due to an unconscious desire to bring about this result. In case of the first two or three re-arrangements, the saving is so considerable that it could hardly be due to any such influence. Then too, the regular decrease in the numbers would hardly occur if that were the case. Nevertheless, to test this point, thirty experiments were made in the following manner:

Six sixteen-syllable series were written upon a sheet of paper. On the back side of the sheet six derived series were written. Six sheets were thus prepared for each of the five transformations described above, making 30 in all. The sheets of each group could not be distinguished from each other on the front side; and they were shuffled and laid aside until the memory of what individual syllables occurred in a given derived series had vanished. Then the series on the front side of one sheet of paper were learned; and after twenty-four hours those on the back side of the same sheet. The time necessary for learning the separate series was noted, but the results were not reckoned up until the whole thirty sheets had been finished. Then these experiments were found to corroborate the results of the earlier ones. Skipping one, two, three, and seven syllables, the saving in learning the derived series was 110 sec., 79 sec., 64 sec., and 40 sec. respectively, the mean time for learning the original series, as before, being about 1266 sec. Deriving the new series by permutation, the re-learning required a mean application of 5 sec. more than the original learning, the mean time for the latter being 1261 sec.

Putting the two sets of results together to obtain a possible balance of disturbing influences, Ebbinghaus estimates the results of the eighty-five experiments in percents as follows.

Strength of association, as measured by the saving of time in re-learning six sixteen-syllable series:

Between contiguous members	33.3 per cent.
Skipping one syllable	10.8 "
" two syllables	7.0 "
" three "	5.8 "
" seven "	3.3 "
With permutation	0.5 "

Ebbinghaus emphasizes the interesting fact that, when the derived series were formed by permutation of the syllables, retaining only the initial and the final members of a series, no saving was shown. The new series formed of the identical syllables originally used could be learned in scarcely less time than an entirely new series. But a series derived by skipping seven syllables could be learned in noticeably less time than an entirely new one. As seven was the number of syllables that Ebbinghaus could learn by a single repetition, it might seem that the associations described were due to the fact that seven syllables were embraced in consciousness at once. But seven syllables were skipped, thus showing association between the first and the ninth members of a series. Moreover the numbers representing the strength of the association were so great and the gradation of them of such a nature, that it seems probable that the bonds of association extend even to more distant members of the series. If there are associations, argues Ebbinghaus, extending to more syllables than can be embraced in one act of consciousness, then these associations cannot be explained simply on the theory that presentations included in one act of consciousness tend to arouse each other.[1]

He also experimented to determine whether there are bonds of association extending backward. He took six sixteen-syllable series and made ten experiments with series derived by mere inversion of the syllables, and four with series derived by inversion and skipping one syllable. In the first case the saving in re-learning amounted to 12.4 per cent.; in the last to 5 per cent.

Further experiments were made to determine the effect of many repetitions. The result showed that while an increased number of repetitions strengthens all the associations, the stronger bonds between neighboring members of a series are strengthened more rapidly than the weaker ones between remote members.

The general results of this whole study of association are summarized as follows:

"In the impressing and strengthening of a series of ideas by numerous repetitions of the same, inner bonds or associations are formed between all the individual members of the series. Every member of the series preserves a certain tendency in case of its return into consciousness to bring the other members with it.

"These bonds or tendencies are of different degrees of strength in several respects. For the remote members of the original series they are weaker than for neighboring members, for definite distances backward, weaker than for the same distances forward. In case of increasing number of repetitions the strength of all the bonds increases. But the already stronger bonds between the neighboring members are by this means considerably more quickly strengthened than the weaker bonds between more distant members. Therefore the more the number of repetitions increases, so much the stronger become absolutely and relatively the bonds of the members immediately succeeding each other, and so much the more exclusive and predominant becomes the tendency of each member when it returns into consciousness to bring after it that one which in the repetition always immediately succeeded it."[1]

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[1] Compare Wundt's experiments on the *Umfang* or extent of immediate consciousness for successive impressions, Physiol. Psych., 2te Aufl., Bd., II., p. 213 seq.

From the study of his results Ebbinghaus found indication of a remarkable rhythm of the attention. A series learned in a proportionately short time was followed as a rule by a series learned in a relatively long time. "There seems to be," he says, "a kind of periodic oscillation of the mental susceptibility or of the attention, in which the increasing fatigue appears in variations about a gradually shifting middle position."[2] Nearly all the experiments showed this oscillation. The results of a typical group of experiments were as follows: In eighty-four experiments with six sixteen-syllable series the mean time for learning the 1st series was 191 sec.; for the 2d, 224 sec.; for the 3d, 206 sec.; for the 4th, 218 sec.; for the 5th, 210 sec.; for the 6th, 213 sec. Representing these results diagrammatically with the number of seconds marked off on the ordinates and that of the series on the abscissas, the curve will be the following:[3]

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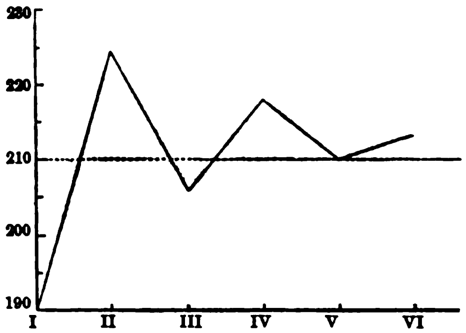
[1] pp. 160, 161.
[2] p. 60.
[3] p. 58.



In experimenting with derived series, however, the phase of the rhythm was changed, and the even numbered series were learned quicker than the odd.[1]

Such in brief outline are the results of Ebbinghaus's laborious and painstaking experiments. Fully to appreciate his scientific rigor and subtle analysis one must read his own work. Some of his experiments have revealed wholly new facts, and others have added a most needful accuracy to what was indefinite. To the latter class belong the experiments on the unconscious persistence of impressions. Thoughtful observers have believed that things forgotten were not wholly lost; the Herbartians have imagined a mysterious subconscious glimmer in the soul; Richet a trace in the brain substance; Luys and others a persisting vibration; Maudsley a

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[1] Ebbinghaus explains this by an ingenious hypothesis of indirect, unconscious association. When the first derived series, made up of syllables 1, 3, 5, etc., is learned, there is a tendency for these syllables to bring with them also syllables 2, 4, 6, etc., though too weak a one to bring them into consciousness — merely an inner excitation, such as occurs when one tries in vain to recall a forgotten name. This, however, strengthens the sub-conscious bonds of the even numbered syllables, and if the derived series which they form is learned *after* that composed of the odd syllables, it is learned more easily. "Inner bonds are formed between successive syllables unconsciously aroused, as between successive conscious syllables, only of course of less strength." — (p. 168.)

"neurotic pattern"; James a "path of association"; Ribot and Wundt a "functional disposition"; and some of the intellectualists have given up the attempt to name it altogether. But whatever it is, the experiments of Ebbinghaus bring it under the yoke of numerical determination. The curve of forgetting for long periods of time has also been determined; and the work upon association has put an experimental foundation under the Herbartian doctrine that each member of a series is linked not only to the following member, but also in lessening degrees to those more remote.

All psychophysic experiments, especially those requiring comparison and those upon the time-sense and the like, involve memory. The experiments of Wundt[1] and his pupils[2] upon the *Umfang*, or extent of immediate consciousness of successive impressions, concern directly what Richet calls the "elementary memory".[3] But to Ebbinghaus belongs the credit of first performing a series of scientific experiments with special reference to the phenomena of memory. His success in testing, by an indirect method, the association of ideas in a series, suggests the possibility of extending the experimental method to apparently inaccessible psychological problems. Useful as his method proved, it was not, however, the simplest one. The learning of nonsense syllables is a complicated psychological process; and while such syllables are free from the associations that cluster about significant material, nevertheless, some syllables are easier than others on account of the eye and ear memories and the muscle memory of the vocal organs. A simpler method is to test the power of recognizing a sense-impression when repeated. It is a well-known fact that such recognition is easier than ordinary recollection without repetition of the external impression. Thus we distinguish many more shades of color than we can picture in imagination; and we generally can understand more words of a language than we can use. H. K. Wolfe adopted this simpler method in investigating the memory for tones. He experimented as follows.[1]

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[1] Op. cit., p. 213 seq.

[2] Especially Dietze, Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinander folgenden Schalleindrücken, Phil. Studien, II., 362—394. Also Cattell, Ueber die Trägheit der Netzhaut und des Sehcentrums, Phil. Studien, III., 94—127; and Hall and Jastrow, Studies in Rhythm, Mind, XI., 55—62.

[3] Loc. cit., p. 568.

Nearly 300 vibrating metal tongues, giving notes through five octaves, were used. A tone was given, and after an interval determined upon beforehand, either the same tone was repeated or a higher or lower one was sounded. The listener wrote one of two answers—"same" (*gleich*) or "different" (*verschieden*). If the answer was the latter, a further judgment was made whether the second tone was higher or lower than the first, or the subject could answer "doubtful." The difference of tone amounted to 4, 8, or 12 vibrations a second, and was kept constant during a group of experiments.

The main point of the research was to determine the influence upon memory of the time-interval between the impressions to be compared. This was varied from one second to 30, 60, or 120 seconds. The greatest accuracy was found when the interval was about two seconds. Three reasons, he thinks, may account for the failure to judge correctly when the interval is less than this: 1st, as we are apt to overestimate short intervals of time the attention may not be focused when the second sound is heard; 2nd, there may not be time, when the interval is very short, fully to perceive the two impressions; or 3d, it is possible that an excitement of the sense-organ persisting from the first impression intensifies the second, and hence the latter is incorrectly perceived.

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[1] See Ueber das Tongedächtniss, Philos. Studien, III., 4.

As the time was increased beyond two seconds, his experiments showed that the curve of memory fell pretty regularly until the interval reached a point somewhere between 10 and 20 sec. Here with each subject Wolfe found a point where forgetting seemed to be retarded or to cease altogether. In case of W. and L., the two subjects with whom the largest number of experiments were made, this swell in the memory-curve occurred, with the former, at an interval of about 15 sec., with the latter at an interval between 20 sec. and 25 sec. Beyond this point the curve falls off still more rapidly with increasing time. But some of the subjects showed traces of possibly a second swell in the curve at an interval from 30 sec. to 50 sec. While smaller irregularities in the curve disappeared with a larger number of experiments, over 4,000 experiments in case of L., and 12,000 in case of W., showed the first retardation point tolerably clearly.

The results in case of these two subjects with whom the largest number of experiments were performed, may be expressed approximately, as Wolfe estimates, in the following simple mathematical formula: The ratio of the right and wrong cases is inversely proportional to the logarithms of the time-intervals[1].

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[1] Letting r = the correct judgments, f = the errors, and K and c = individual constants; the formula is

$$\frac{r}{f} = \frac{K}{\text{Log. } t} + c,$$

or

$$r = \frac{Kf}{\text{Log. } t} + cf.$$

Ebbinghaus's formula was similar. (See *supra*). It is an interesting question whether this law would hold with other experimenters and for all the senses. Dr. Jastrow found in testing the eye and hand

Thus Wolfe's experiments with short intervals corroborate those of Ebbinghaus with longer periods.

The results of Ebbinghaus's study, as has been said, have primarily only an individual value. So far as I am aware, there is no record of any adequate attempt to test these results with many different individuals. A pupil of Kraepelin, however, Dr. Oehrn, has made a suggestive study of individual psychology[1]; and in reference to memory he used substantially the method of Ebbinghaus, although in part substituting number series for nonsense syllables. But his experiments thus far reported have given no satisfactory results as far as memory is concerned, except to corroborate the testimony of others, that in the performance of any mental function there are great individual variations, and to show that in such experiments the mean variation is a direct measure of the share of the attention in the process. Thus, in learning the tedious nonsense syllables, the average mean variation for his ten subjects was nearly six times as large as in the more interesting process of adding[2].

Upon one point, however, the experiments of Ebbinghaus have been supplemented by a research

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memories for measurements, that "in both cases the memory is extremely accurate and is almost as faultless after the lapse of a few days as of a few minutes." See The Perception of Space by Disparate Senses, Mind, XI., p. 552. The same article contains experiments upon the relative accuracy of the eye-arm-and-hand memories for space perceptions.

Beaunis has reported the provisional results of somewhat similar experiments upon muscular sensations. He found three phrases in the vanishing of muscular memory when tested by the power to reproduce the extent and direction of movements: 1st, *the phase of conscious memory;* 2d, *the phase of unconscious memory,* where conscious memory has vanished, but it is still possible to reproduce the given movements; 3d, *the phase of total obliviscence.* See Rev. Philos., XXV, pp. 369—574.

[1] Experimentelle Studien zur Individualpsychologie. Inaug. Dis. Dorpat, 1889.

[2] See review of his paper in the experimental section of this number. ED.

upon many individuals. Mr. Jacobs and Mrs. Bryant have tested the powers of school children in reproducing numerals and letters after once hearing them[1]. Their most important experiments were performed with the boys of the Jews Free School and the girls of the North London Collegiate School. Numerals (omitting *seven*) and letters (omitting *w*) were employed as more uniform in pronunciation and rhythm than nonsense syllables. The letters or numerals were dictated *once* in a monotonous tone at the rate of 120 sounds a minute, and the pupils repeated orally or wrote as accurately as they could the series given. The following are the most important results of this interesting research.

In the first place, the mental "span"—as the experimenters called the ability measured by the maximum number of syllables that could be repeated after once hearing them—showed a decided increase with the age of the pupils. Boys of 11 years could repeat in this way 6.5 numerals and 5.5 letters; of 12 years, 6.8 numerals and 5.7 letters; of 13 years, 8.8 numerals and 7.9 letters. The following table shows the results of a large number of observations at the girls' school:—

Age,	8	9	10	11	12	13	14	15	16	17	18	19
Number of subjects,	8	13	19	36	41	42	42	72	66	50	30	14
Average number of numerals,	6.6	6.7	6.8	7.2	7.4	7.3	7.3	7.7	8	8	8.6	8.6
Average number of letters,	6	7	6.6	4.6	6.5	6.7	6.7	7.4	7.9	7.3	8.2	7.9

Again the relation of this mental span to the pupil's rank in the class was clearly marked. "As a rule high span went with high place in the form." In a class of twelve-year-old boys the average span for numerals was 9.1 for the first ten, 8.3 for the next and 7.9 for the lowest ten. In another class of thirty boys of the same age, the first ten could grasp 7.6 numerals, the second ten 7.1, and the lowest only 6.3.

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[1] Mind, XII, pp. 75 seq.

Similar results were found at the girls' school. The first half of a class usually showed a higher span than the second half.[1]

Mr. Galton, Prof. Bain, and Mr. Sully made similar observations on the memory-power of idiots. Nine of the best girls among the inmates of one asylum were tested with numerals. One could recollect 2 figures, one 3, three 4, and the remainder 5. And at another asylum where the inmates were of higher mental capacity, some were found with a higher span.

Oliver Wendell Holmes experimented in this manner some years ago[2], and was surprised to find how frequently mistakes are made in repeating between seven and ten figures or letters. An individual may sometimes be found, he says, who can repeat a larger number, and he refers to the famous cases on record where persons have repeated sixty or more unconnected words after once hearing them[3]. But the latter, apart from a few anomalous cases, are hardly to be considered here; for usually the words are repeated slowly and the hearer links them by mnemonic devices.

The amount of this memory-span evidently bears some relation to the mental capacity; and Dr. Holmes has suggested that in such experiments we have "a very simple mental dynamometer which may yet find its place in education."[4] Certainly this test or some other direct observation of memory should be used as a test for cerebral fatigue; for experiments show that memory is very sensitive to changes in physical condition.[1]

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[1] Dr. Cattell found that a distinct shortening of the time required for the mental process of association accompanies growth and education. See Mind, XIV, p. 234.

[2] Op. cit., p. 32.

[3] For an account of cases of remarkable memory see Huber, op. cit., p. 36 seq; also Jour. of Spec. Phil., V, pp. 6–26.

[4] Loc. cit, p. 33. Mr. Jacobs makes the same suggestion. See loc. cit., also Mind XI, p 54.

Much experimental study has been devoted to the association of ideas. The most extended researches have been made by Trautscholdt[2] and Cattell.[3] The former sought to determine the time required to perform an ordinary act of association, and classified the results of a large number of experiments according to the nature of the association. The latter has extended the research to various forms of association, and has obtained further statistics of its nature. These important studies are too well-known to require extended comment. But as yet, valuable as they are, they have thrown no great light upon memory; for the experiments have been comparatively few in each form of association investigated; and in such experiments, the mental processes measured are inevitably complicated, voluntary recollection being mixed with spontaneous, and both with acts of judging.[4] They are, however, suggestive; they illustrate the complexity of the process of association, and show the important rôle of obscure processes in reproduction.

The average time for the mental process of association was found by Trautscholdt to be 0.727 sec. It is noteworthy that this time is approximately the interval that numerous observers have found can be most accurately reproduced. From this fact

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[1] Cp. one of Galton's tests: La fatigue mentale, Revue Scientifique, Jan. 26, 1889, p. 101.

[2] Philosophische Studien, I, p. 213 seq.

[3] Reported in Mind, XII, pp. 68—74 and XIV, pp. 230-250. Cf. also his earlier experiments on the time required for verbal operations; Mind, XI, pp. 63, 220,, 377, 524.

[4] Cp., the remarks by Cattell's subjects, Mind, Vol. XIV, p. 244 seq. See also Dr. Münsterberg's criticism of Trautscholdt's experiments, Beiträge z. experimentellen Psychologie, Heft 1, p. 87 seq.

Wundt concludes "that a rate of about three fourths of a second is that at which processes of association are most easily performed, and which therefore, in the reproduction of objective time-intervals we involuntarily seek to equal by shortening longer periods and lengthening shorter ones.[1] Remarkably enough this time nearly corresponds to the time required for the swinging of the legs in rapid walking. It does not appear improbable that both the psychic constants of the average speed of reproduction and of the most accurate time estimate have been formed under the influence of the most habitual bodily movements, which have also determined our inclination to divide greater periods of time rhythmically.[2]"

Dr. Alfred Lehmann, of Kopenhagen, has studied another aspect of the subject,[3] and attempted to prove that all association can be explained by the single law of contiguity. Like Wolfe, he employed the method of right and wrong cases, and tested the power of recognition; but he experimented with sensations of color, using different shades of gray produced by means of rotating disks, partly black and partly white. In part his experiments supplement the work of Wolfe, and tend to show that the same laws apply to recognition by sight as by hearing; for, varying the time-interval, from 5 sec. to 120 sec., five series of 30 experiments each showed that as time increased, the number of correct judgments decreased with one observer from 30 to 17; with the other from 21 to 17. The experiments give some support to the author's thesis in regard to the laws of association.

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[1] Cp., however, the experiments reported by Stevens, Mind XI, p. 393 seq.

[2] Op, cit., II, p. 286. But see also Galton, who used a different method and found the rate of association to be about 50 ideas a minute; Enquiries into Human Faculty, p. 188 seq.

[3] Ueber Wiedererkennen, Philos. Studien, V, 96—150.

Among experimental methods should also be mentioned hypnotic tests of memory. Perhaps the most important result of these studies has been to demonstrate the existence and importance of what Bernheim calls "latent memories,"[1] which account for the performance of suggested acts after long intervals of time. Space is not left for us to consider here the relation of memory to the hypnotic sleep; but an account of the main facts may be found in the works of Delbœuf,[2] Janet,[3] Bernheim[4] and others, and in the convenient little résumé by Dr. Dichas.[5]

III.—*Conclusion.*

In this brief study of memory from the earliest mythology to recent experimental researches, representative philosophers have been chosen. If the more idealistic and transcendental theories may sometimes seem to have been slighted, it is because an adequate presentation would lead too far into metaphysics rather than lack of appreciation of them. The aim has been to avoid the wider region of philosophical inquiry, upon the borders of which we have repeatedly found ourselves. Still, limiting our inquiry, a multitude of problems arise. Apart from the special questions of cerebral localization,[6] of the trustworthiness of memory, the relation of memory proper to habit or organic memory, the unity of personality, the nature and conditions of "latent memories,"[1] and the processes that occur on the borderland between memory and habit,[2]—apart from these and the more technical questions that cluster around the pathological and biological aspects of the subject, there are yet others. What is the relation of the sensorial after-image, to the memory after-image and of the latter to the ordinary mental image? What are the time-limits and physical conditions of the primary memory? Are the phenomena of recurrent sensations mentioned by Henle and Fechner due to peripheral or central causes? What is the relative accuracy of memory in the field of the different senses? Is there a rhythm in retentiveness as such, or are the variations in the curve of forgetting like those found by Wolfe due to fluctuations of the attention and the like? Do we always remember forwards and not backwards?[3] What are the obscure links in the process of association that the associationist atomism loses sight of? What is the nature of that matrix of presentations which Mr. James Ward calls the "presentation continuum?" and what its relation to the "memory continuum?"[4] Can the memory as such be trained, or do so-called memory-exercises really train only the attention?[5] What degree of memory have animals? What is the genesis of memory in children? and what the relation of great memory to the development of the intellectual powers? These and a host of other problems invite research.

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[1] De la suggestion et de ses applications à la thérapeutique, I., Ch. VIII. Paris, 1886.

[2] La mémoire chez les hypnotisées, Rev. Philos., May 1886, p. 441 seq.; also Le sommeil et les rêves.

[3] L'automatisme psychologique, Paris, 1889; also Rev. Philos., XXV., pp. 238–279.

[4] Op. cit.

[5] Étude de la mémoire dans ses raports avec le sommeil hypnotique. Paris, 1887.

[6] For an enumeration of important problems concerning aphasia see The Pathology of Sensory Aphasia, by Dr. Starr. Brain, July, 1889, pp. 95, 96.

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[1] See Bernheim, op. cit., I., Ch. VIII.

[2] See Beaunis, loc. cit., who found a phase of unconscious muscular memory.

[3] See Bradley: Mind XII., p. 579 seq.

[4] See Encyc. Brit., Art. Psychology, p. 61.

[5] Several recent writers consider the pedagogical aspects of memory. See especially Kay: "Memory, What it is and how to improve it;" C. G. Leland: "Practical Education;" Fauth: Das Gedächtniss; and Eldridge-Green: "Memory, its Logical Relations and Cultivation."

Finally we come again to the deeper philosophic questions. What is the genesis of the power of recognition? and what the explanation of this supreme mystery of memory?[1] Have we not in acts of recognition and comparison forms of consciousness that are not mere functions of its individual elements? What is the relation of attention to the conscious train?

The last question brings us to the front of the battle between the Associationists and the Apperceptionists. The former, under the lead of Bain in England and supported to a considerable extent by empirical studies in France[2] find nothing in the rise and succession of our ideas that may not be explained by the principles of association. The latter with Wundt and Ward at their head lay special stress upon attention and find higher conscious activities that get no adequate explanation from the laws of association.[3] Wundt maintains that apperception, the primal form of voluntary activity, is an essential characteristic of consciousness from the first, that it depends on the whole nature and past of consciousness, and that the will in its function of active apperception directs the conscious train. Recently the contest has extended to the experimental field. Dr. Hugo Münsterberg reports an extended series of experiments upon complicated mental processes the results of which in his opinion clearly refute this apperception theory.[4]

Thus the study of memory—as every psychological study sooner or later must—leads to the subject of attention, and to the deeper mysteries of consciousness; and here for the present this historical paper must end.

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[1] Space forbids a discussion of theories of recognition and localization. For an excellent discussion of the subject and criticism of recent views, see an article by Fouillée, Rev. des Deux Mondes, July 1, 1885.

[2] See especially Binet's Psych. du raisonnement.

[3] See Ward, loc. cit., p. 41 seq.; and Wundt, op. cit., Bd. II, Ch. XX.

[4] See Op. cit., p. 64 seq.

APPENDIX.

In closing the historical portion of this study I wish to thank the officials who have aided me at the Harvard and Johns Hopkins Libraries, at the Boston Public Library, the Peabody Library of Baltimore, and the Surgeon General's Library at Washington. I am also under obligation to many others, especially to Pres. G. Stanley Hall, at whose suggestion this work was begun, and to Dr. E. C. Sanford for invaluable suggestions.

I append a bibliography of the most important literature of the subject so far as I have been able to gather it. Only a few titles upon the Association of Ideas are given; and works upon mnemonics, except a few of special interest, have been omitted. Those interested in the subject can refer to the list of books given in Ersch and Gruber's encyclopedia, article *Gedächtnisskunst*, and to G. S. Fellows' bibliography in the American edition of Middleton's "Memory Systems New and Old." Literature upon the pathology of the subject also has been omitted for the most part. For this the articles upon *Aphasia* and *Amnesia* in the catalogue of the Surgeon General's Library should be consulted. Nearly all works upon the general subject of psychology have chapters upon memory. A few of the most important are mentioned in the following list as representative. Nearly all works upon Hypnotism also have some bearing upon memory. For the voluminous literature of this subject see Max Dessoir's *Bibliographie des modernen Hypnotismus*, Berlin, 1888.

Many references of minor importance already given in foot-notes are omitted. In a few cases I have mentioned works that I have not seen when I had reason to believe them of value.

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PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

Experimentelle und pathologisch-anatomische Untersuchungen über die optischen Centren und Bahnen. (*Neue Folge*). DR. C. v. MONAKOW. Arch. f. Psychiatrie, Bd. XX, Heft 3.

This paper is based on the detailed study of some five brains, three of them from dogs, from which the visual areas had been removed by Munk. These dogs were observed by Munk subsequent to the operation for considerable periods of time and when killed the brains were turned over to v. Monakow for histological examination —an example of that sort of co-operation in scientific investigation which will assuredly yield good results. The principal discussion relates to the true limits of the cortical visual areas (in dogs), the path connecting this region with the primary optic centres in the corpora geniculata externa, pulvinar and anterior corpora bigemina, and the nature of the connections in these centres on the one hand with the cortex and on the other with the retina. (In considering the latter points the results obtained from dogs, cats and rabbits are combined). In the dogs' brains the cortex for some millimeters cephalad of the visual area, as mapped by Munk, had degenerated, whereas the other portions of the surrounding cortex remained normal up to the edge of the cut. This difference in behavior is regarded as due to the fact that in the operation the coronal fibers for this anterior portion are almost of necessity injured and injury to these would cause the condition observed. In one case also the gyrus fornicatus was found to have a portion of the corpora geniculata externa dependent on it. This observation needs confirmation. At present v. Monakow contents himself with suggesting that the true limits of the visual area should be extended 4 or 5 mm. cephalad, of those given by Munk, especially along the mesial boundary. The fibers connecting the visual cortex with the primary optic centres run in the posterior portion of the internal capsule. When the cortex is removed by the knife there still remains a not inconsiderable portion of the gray matter in the depths of the sulci. As there was every reason to consider this as a part of the visual cortex and as at the same time it did not appear to be functional in operated dogs, Munk has assumed that it has degenerated as a consequence of the injury. v. Monakow however finds it in fairly good condition in several cases and hence some other explanation is needed for its inactivity.

Following the removal of the visual area changes take place in all three primary centres. In dogs and cats, the degeneration is found in the entire ventral and the cephalic portion of the dorsal nucleus of the corpora geniculata externa; in the caudal and ventral portions of the pulvinar and in the superficial gray and middle fiber layer of the anterior corpora bigemina. (By way of explanation it should be understood that v. Monakow distinguishes five cell groups in the corpora geniculata externa. They form a dorsal group of two lying above a ventral group of two, which in turn is above the single ventral nucleus. The retina is specially connected with the caudal-dorsal nucleus while the other four are connected with the cortex. They are the frontal-dorsal caudal-ventral, frontal-ventral, and the ventral nuclei). In newborn dogs and cats the atrophy passes beyond the primary centres and involves the optic tracts, the optic nerves and the cells of the granule layers of the retina. In rabbits there is atrophy of the entire dorsal with slight atrophy of the ventral nucleus of the corpora geniculata externa, while the external fibre layer remains intact; atrophy of the ganglion cells and ground substance of the pulvinar and, in the anterior corpora bigemina of the middle fiber layer.

Section of the visual fibers in the internal capsule gives peripherally the same result as the removal of the visual area. At the same time there is an ascending degeneration which involves the large cells of the third layer of the cortex and the nerve networks in the region of the third and fifth layers.

Enucleation of the bulbi oculorum, in newborn animals is followed, in dogs, by atrophy in the gelatinous substance of the lateral zone of the corpora geniculata externa, together with that of the ganglion cells, especially in the dorsal-caudal nucleus of this body, also in the dorsal-caudal portion of the pulvinar. In the dog the disturbances in the anterior corpora bigemina are doubtful. The changes in the cat are similar to those in the dog save that the disturbance in the anterior corpora bigemina are more evident. In the rabbit finally the gelatinous substance of the lateral zone of the corpora geniculata externa atrophies, the cells of this body remaining fairly normal. The pulvinar shows no changes (?) but in the anterior corpora bigemina there is atrophy of the superficial layer of fibers with disappearance of the cells from the superficial gray layer. After removal of one of the anterior corpora bigemina, in the cat or rabbit, the tractus opticus becomes smaller and in the optic nerve the finer fibers degenerate, while the coarser ones remain intact (v. Gudden and v. Monakow). In newborn cats, in which an entire hemisphere has been removed and also the tractus opticus of the same side cut, a group of cells in the caudal-dorsal region of the corpora geniculata externa remains, while all other groups, together with those of the pulvinar, disappear (v. Monakow). After section of one optic tract in a rabbit there is atrophy in both nervi optici (insignificant, of course, in that of the same side), and in the corresponding portion of the retinae disappearance of the large ganglion cells of the deep layer. (Ganser).

These facts v. Monakow attempts to knit together so as to present in a scheme something of the relation of cells and fibers along the entire line of the optic pathway. In doing this he considers it as proved that, in newborn animals at least, section of systems of fibers causes them to degenerate in both directions, that after such a section certain groups of ganglion cells are fully destroyed while others are only slightly affected. He also accepts the two categories of cells according to Golgi, but agrees with Forel in considering those, the axis cylinder of which soon loses its identity and forms a network, as central or intermediate cells, serving to put into connection those of the first category. Physiological connection between the branches of a net is considered as due rather to the contiguity than the continuity of the finest branches. The cell groups that do not degenerate on the section of the fibers are identified as intermediate cells (second category). Not to go into the details of the scheme we give his general statement of the result. "In each optic centre one system of fibers arises and one ends and the several systems of projection fibers are united with one another by intermediate cells. Both in the primary and in the secondary portions of the optic pathway two fiber systems run parallel and in opposite directions and groups of principal cells regularly alternate with intermediate ones." Or putting the facts in another way: in the centripetal direction we have (in the rabbit), the large ganglion cells of the retina, their axis cylinder processes passing in the optic nerve and terminating in a net-work in the lateral zone of the corpora geniculata externa. Here they make indirect connection with the intermediate cells which in turn are connected with the principal cells of this body and of the pulvinar. From these latter points fibers run to the fifth (and third) layer of the cortex in the visual area, there forming a network that connects with the cells forming the above mentioned layers and these in turn connecting with other elements (solitary cells) of the cortex. In the centrifugal direction we have the large pyramid cells of the third cortical layer, their axis cylinder prolongations passing ventrad as the fibers of the visual area and forming a network in the corpora geniculata externa (where?) and in the anterior corpora bigemina and through the intermediate cells in the latter, making connection with the principal cells in the superficial gray layer. The axis cylinder prolongations of these last give origin to the fine optic fibers which form a network in the retina and connect there with the granule layers.

This scheme is put forward by the author as tentative, but he expresses the hope that in the main it may be found correct. The idea of double tracts throughout the optic pathway is certainly striking, but in view of the problematical nature of the retina and optic nerve from a morphological standpoint, certainly worth further study. It must be remembered that as presented the scheme applies strictly to rabbits only.

On his way to these conclusions v. Monakow makes a number of interesting observations some of which may be briefly referred to. Goltz has pointed out that the dog without eyes differs much from the dog without visual centres, the latter exhibiting a marked disinclination or inability to go up and down steps or jump from a height, acts which the dog merely without eyes does with ease. A dog without eyes, which v. Monakow observed, and about whose general condition of health there could be no doubt, learned to go up and down stairs only with much difficulty and could not be persuaded to jump from a low bench. This same dog followed the movements of the children who kept him with such ease, was so active in play and in so many ways masked his defect of vision that v. Monakow refers to the case of the rabbit which had lost its visual centres on both sides and yet acted quite as if it could see (v. Gudden), remarking that the tests were such that, if applied to the blind dog just mentioned, they would prove that he too could see. This criticism is however not conclusive. There seems little doubt that certain birds can see without the aid of the cerebral hemispheres and among the cold blooded vertebrates the primary optic centres appear quite independent. It is still a fair field for investigation to determine whether among the mammalia such independence can exist.

On the anatomical side v. Monakow notes that in the vertebrate series as we descend, the anterior corpora bigemina or their homologues, the optic lobes, become more and more important as optic centres until they are finally the exclusive centres for vision. As regards the history of the degenerative process v. Monakow finds that in the pulvinar and the anterior corpora bigemina the degenerative process after removal of the visual area first attacks the ganglion cells and only secondarily and later is the ground substance involved. Further, that the stellate cells are comparatively early productions in the process, being absorbed in the later stages.

A continuation of the paper is promised.

Makroscopische pathologisch-anatomische Hirnpräparate. DR. C. v. MONAKOW. XIV Wanderversammlung südwestdeutscher Neurologen und Irrenärzte, Mai, 1889. Abstract in Neurolog. Centralbl, No. 13, 1889, by Dr. L. Laquer.

v. Monakow exhibited the following preparations:

1. Brain of dog from which on the day of birth there were removed the entire occipital and a portion of the temporal lobes on the left side. Death after eight weeks, corpora geniculata externa, pulvinar and corpora geniculata interna on the left side much atrophied. Left optic tract up to chiasma considerably reduced in volume. Both nervi optici slender.

2. Brain of a dog from which the larger part of the right cerebral hemisphere was removed three days after birth. Besides the frontal end and the olfactory lobe only fragments of the gyrus fornicatus and sigmoideus and of the temporal lobes remained behind. The internal capsule was completely severed. Death after 8½ months. There was found great shrinkage of the thalamus opticus, corpora geniculata externa and corpora geniculata interna on the right side; evident flattening of the right anterior colliculus of the corpora quadrigemina and the right corpus mammillare. Right pyramid had completely disappeared and the pons on the right side was flattened. Right optic tract as far as the chiasma reduced to half the size of the left one. Both nervi optici small but not macroscopically very different from one another. The region of the nuclei of the columns of Goll and Burdach slightly depressed on the left side; corpus callosum very thin. The cerebellar hemispheres quite normal and similar but the hemisphere on the right side had expanded somewhat into the cavity left by the removal of the occipital lobe on that side.

3. Human brain extensive region of softening in the left occipital lobe. Patient a painter, 68 years of age, who suffered from hemianopsia and alexia. The medullary substance in the region of the angularis, the first occipital convolution and the caudal-dorsal portion of the præcuneus for the most part destroyed and absorbed, giving rise to an extensive cyste. The softening nowhere reached the sagittal fibers. Posterior cornu much distended, cuneus and the second and the third occipital convolutions intact. The same was true for the ventral portion of the bundle of Gratiolet. The cortex of the gyrus angularis appeared quite normal. Secondary degenerations were observable in the dorsal portion of the bundle of Gratiolet on the left side as far as the lateral fiber layer of the pulvinar. Considerable secondary reduction in the pulvinar and corpora geniculata externa on the left side. Atrophy of the arm of the anterior colliculus and of the caudal portion of the left tractus opticus.

The right optic nerve as large as the left but somewhat gray on its mesial side.

4. Brain of an idiot about 28 years of age who at the age of two had experienced an embolism of the left arteria fossæ Sylvii. On the left side the first temporal convolution is wanting and the ventral portion of the left parietal lobe, the medullary substance having especially suffered. Lateral ventricle much distended. Secondary atrophies; left pyramid very slender. The median nucleus of the thalamus opticus and the tuberculum anterius much diminished, while the pulvinar and the corpora geniculata externa corresponding with the fairly normal condition of the occipital lobe are almost as well developed as on the right side. The left corpus geniculatum externum has however almost completely disappeared. The corpora striata are normal on both sides. v. Monakow then expressly called attention to the fact that the intimate connections shown by him to exist between the various portions of the cortex and the inter and mid-brain were true not only for the rabbit, but also for the dog and man, being indicated by the secondary degenerations following cortical lesions. The dependence of the corpora geniculata externa on the cortex of the temporal lobe is for the first time described for man, but corresponds with observations previously made on both the rabbit and cat. The cases 1 and 3 also illustrate the necessary degenerations of the primary visual centres after the injury of the occipital lobes.

Ueber Befunde bie Erkrankung des Hinterhauptslappens. HOELE. Berliner Gesellschaft für Psychiatrie und Nervenkrankheiten. Sitzung vom Juli 8, 1889. Rev. in Neurolog. Centralbl. No. 14, 1889, by Hadlich.

The author presents observations of three cases in which there was disturbance of vision during life and the autopsy showed lesions in the occipital lobes, with which these symptoms were associated. At the same time the optic pathway from the cortex to the eye was more or less involved showing signs of atrophy. The results were presented as a contribution to the localization of vision in the occipital lobes.

Ueber die centralen Organen für das Sehen und das Hören bei den Wirbelthieren. HERMANN MUNK. Sitzungsber. d. Königl. Preuss. Akad, d. Wiss. zu Berlin. XXXI. 20 Juni, 1889.

This condensed account of Munk's views on some disputed points forms the fifth and closing paper of a series which commenced in 1883. In it he presents, besides his critical remarks, the main results of further work on dogs, monkeys and newborn rabbits.

In operating dogs and monkeys he recommended that the portion of the brain removed be cut out in one piece, one advantage of this method being that it enables the experimenter to see pretty accurately what the extent of the initial lesion is. The results are similar to those obtained by his previous procedure. These in the main have been corroborated by Sanger Brown and E. A. Schäfer and by Vitzou from the experimental side, while from the pathological side Nothnagel furnishes evidence for a similar relation of the cortex in man. Munk extends his results to the lower mammals, on the ground that though the visual centres are not well made out there, yet in rabbits, guinea-pigs and rats, the removal of an entire hemisphere causes contralateral blindness. In discussing the results of Schrader on pigeons (see AMER. JOUR. PSYCHOL., vol. 2, p. 471) which are opposing—for Schrader claims that pigeons without the forebrain can still see—Munk urges as a chief objection that the cortical substance was not entirely removed, and if some remained it would account for what Schrader observed. Deciding against him, he concludes that the relation of the forebrain to vision is fundamentally different in the mammals and birds (?) to what it is in frogs and fish, and that the former without their forebrain are entirely blind. Further, that in the former the very sensation of light has its seat in the cortex, and that the optic or retinal reflex—the narrowing of the pupil when light is directed on the retina—does not require even the sensation of light for its performance, and cannot therefore be adduced as an argument.

The final portion of the paper is a criticism of Wundt's position on this question. Wundt argues that the (central) cortical elements are functionally indifferent, and the characteristic sensations which they give are due to molecular disturbances of a special sort dependent on the nature of the stimulus which has excited the nerve. In other words, if it were possible to connect one sensory nerve with the cortical centre of another sensory nerve, the sensation following a stimulation of the nerve would depend on the nerve stimulated, not on the centre with which it was connected. Such a view is based mainly on the apparent restitution of function after operations on the cortex, on the possibility of removing large parts of the hemisphere with only slight disturbance of function as a result. The other objection is the theoretical one that either the specific energy of the cortical cells was present from the earliest stages of development, which is highly improbable, or it has been gradually developed, in which case it is not strictly specific. In criticising the above, Munk points out that there is no good evidence for the notion that specific energy of the cortical cells depends on peculiar vibrations of the nerves to which they are attached, and that experiments on new-born animals, which should support this idea if any experiments would, are in fact opposed to it. As to the second objection, it might be urged against the physiological division of labor throughout the entire organism, and is not a special difficulty of the doctrine of the specific energy of nerves.

(The somewhat rigid form which Munk gives to his ideas on localization has always excited opposition because it is opposed to the gradation of function which is characteristic for living things. As regards the visual centres, he will have all mammals like the dogs and monkeys which he has operated; and yet for rabbits, guinea-pigs and rats he admits that the visual centres have not been defined, and for the last two bases his conclusions on those which have lost an *entire* hemisphere. In discussing v. Gudden's rabbit which had lost the posterior portions of both hemispheres, he states that undoubtedly that rabbit could see, but adds that other rabbits which in addition have had the cortex for some two mm. further cephalad removed were blind, and blames v. Gudden for his assumption that he had by his operation completely removed the visual centres. In the case of Schrader's pigeons, the objection is urged as one of great importance, that small portions of the cortex had been overlooked and were not removed. The above is prefatory to the question: what is the significance of the small portions of cortex? The animals in question are not reported as seeing slightly with a small portion of the retina, but guide and control their motions with some skill. Either, then, their sight is better than the portion of cortex remaining would indicate, if the relations are the same as in dogs and monkeys, or a small portion of cortex comes to possess unusual powers as a visual center. Indeed, the attitude of Munk and Flourens with regard to the controlling value of a small part of the cortex is, when viewed in the light of the above experiments alone, by no means so different. Certain it is that the dependence of the centres in the spinal cord on the motor portions of the cortex decreases as we pass from man down the vertebrate series, and this fact suggests the possibility of a similar relation between the primary sensory centres and the cortical areas belonging to them. Such an idea would imply in the lower mammalia some power of the primary centres to act even after the cortex was destroyed. Against such a view Munk protests vigorously. But leaving this point one side for the time, it is most important that some demonstration of the value of residual portions of the cortex should be made, to determine whether the degeneration in the primary centres is quantitatively proportionate to the amount of cortex removed, or whether a small portion of cortex exercises a nutritional influence over these centres which is disproportionate to its size, or, whatever the relation may be, to give it some anatomical basis. REV.).

Ueber den Bau des Säugethiersgehirns. Vorläufige Mittheilung von DR. G. JELGERSMA in Meerenberg, Niederlande. Mit 1 Tafel, Morph. Jahrb., 1889, 15. Rev. in Neurolog. Centralbl., No 14., 1889, by P. Kronthal.

The author describes the intellectual tracts and centres of the medulla oblongata and cerebral axis. The course of the tracts is as follows. Starting from the cerebral cortex and passing through the internal capsule and the two lateral portions of the pes pedunculi, they unite in part with the nuclei of the pons on the same side and in part pass on to the ganglion cells of the nucleus olivaris. Both sets of fibers then cross with the corresponding ones on the other side. The crossed fibers from the nuclei of the pons pass through the brachium pontis, those from the nucleus olivaris through the corpus restiforme, the former going to the cortex of the cerebral hemispheres, and the latter probably to that of the vermis cerebelli. The cerebellar cortex is further connected with the nucleus dentatus. From this arises the superior peduncle of the cerebellum, which in turn, at the level of the corpora quadrigemina crosses with that of the other side, to unite with the nucleus ruber. From this nucleus fibers pass to the optic thalamus and the capsula interna, to finally end in the cortex.

The differences between man and the monkeys with regard to this entire tract are considerable: in man, the great development of the cerebellar hemispheres; in the monkey, the exposure of the corpus trapezoides on the cephalic side, absence of the neucleus arciformis, meagre development of the nucleus ruber, of the superior peduncle of the cerebellum, of the nucleus olivaris and the nucleus dentatus. The difference in the development of the intellectual tract seems to be as great as that of the surface of the brain. In comparing brains of different classes of animals, however, with reference to this tract, it became plain that the high or low organization of the brain could thus be only very roughly determined.

The author correlates the development of the vermis with that of the axial ganglia, and concludes that the function of the cerebellum is psychic. Regarding the formation of the convolutions Jelgersma concludes that it is independent of the forces outside of the brain itself, and that both in the cerebrum and cerebellum the formation of convolutions is due to a localized tendency to superficial growth and mutual accommodation between the gray substance and the conducting white matter.

Monstres Cyclopes. C. Phisalix. Journal de l'Anatomie et de la Physiologie, etc. Janvier, Février, 1889.

The monsters are all mammalian and the cyclopean type is illustrated by one case from man, one from a dog, and two from sheep. The parts concerned are carefully and minutely described with a view to determining to what extent the abnormalities are correlated. The conclusion of the argument is that nutritive, not mechanical causes must be called in to explain these cases; and it is urged that with this point in view monsters should be studied histologically. From the anatomical side it is made out that the cyclopean condition is always associated with arrested development of the fore-brain; and that the relations of the choroid plexus show that the plexuses of the lateral and third ventricles, which in the adult are in connection, have an independent development and become fused later. In the case of the dog sensory branches were found arising from the fourth nerve (patheticus), thus supporting Phisalix view on the spinal type of some of the cranial nerves. (See Am. Jour. Psy., Vol. 1, p. 492.)

Experimenteller Beitrag zur Kenntniss der Hörnervenursprungs beim Kaninchen. Prof. Bumm. Jahressitzung des Vereins der deutschen Irrenärzte 1888. Allg. Zeitschr. f. Psychiatrie, etc. Bd. 45, Heft 5 and 6, 1889.

Four rabbits were operated three days after birth. In *A* and *B* the left acusticus was cut; in *C* the left cerebellar hemisphere removed, and in *D* the vermis. *A* and *B* were killed after three weeks, *C* after six months, and *D* after six weeks. Examination of the posterior auditory root in *A* showed, according to the author, that this root rises from the tuberculum acusticum and the anterior auditory nucleus (terminology of Forel-Onufrowicz) and that both these ganglia are also in connection with the fibres of the corpus trapezoides. Rabbit *B* showed that the anterior auditory root rose (in part) from the cells and network ventrad of the nucleus of Deiters. It is concluded from *C* and *D* that the posterior auditory root is not connected with the cerebellum, whereas the anterior has a partial origin somewhere in the vermis. On the central paths of the auditory fibres his specimens throw no light.

The relations between the superficial origins of the spinal nerves from the spinal cord and the spinous processes of the vertebræ. R. W. Reid. Journal of Anatomy and Physiology, Vol. XXIII, April, 1889.

Taking the spinous processes as his landmarks, Reid, by careful dissection of six subjects, has gotten a series of results which are presented in two tables and two plates. He first determined that the variation in the level of origin between the sensory and motor nerves of the same segment did not exceed three mm. in any case. In making the investigation then the cord was exposed so as to show the posterior nerve roots and at the same time leave half the spinous process in place. Five of the specimens were adult males, one an adult female. The first table gives the upper and lower limit for each posterior root. The second table is condensed from the first to give the maximum variation, and the second plate graphically represents the results in the second table. The result shows great variability in the superficial origin of the spinal nerves when referred to the spinous processes.

On nerve tracts degenerating secondarily to lesions of the cortex cerebri. C. S. SHERRINGTON, M.A. (Preliminary communication). Journ. of Physiology, Vol. X, No. 5., July, 1889.

In 1884 Sherrington worked over some material put at his disposal by Prof. Goltz with a view to finding whether in dogs the location of the lesion in the cord could be connected with the portion of the "cord area" (*i. e.*, that region of the cerebral cortex the removal of any part of which causes a degeneration in the spinal cord) which was removed. He soon found that the appearance of the cord lesion was so dependent on the time elapsing between operation and autopsy that it was first necessary to study the variations due to this factor. In the present paper he returns to the original question of determining "to what extent there is in the pyramidal tracts a grouping of nerve fibers corresponding to the grouping of nerve cells in the cord area of the cerebral cortex." The experiments were mainly on monkeys (Macacus). In these animals the pyramidal tract was found to occupy three fourths of the transverse area of the crusta a little above the pons; and to extend as low as the origin of the cocygeal nerve roots. It was further noticed in the cord of Macacus that from the level of the 2d lumbar nerve roots a considerable portion of the tract lies outside the direct cerebellar tract, thus separating the latter from the periphery of the lateral column. "After a cortical lesion of less than 30 square mm. extent the degeneration in the cord was found scattered over the whole transverse area of the tract." This furnishes a negative answer, therefore, to the initial question. The only grouping among these fibers passing from cortex to cord, was indicated by the fact that the more mesial and anterior cerebral lesions were followed by degeneration which (in the cervical cord) abutted on the direct cerebellar tract, whereas following removal of the more posterior and lateral cortical portions the degeneration about the cerebellar tract was less evident.

Further examination indicated that the "re-crossed" pyramidal fibers were well developed in the monkey in the cervical and lumbar enlargements. The "re-crossed" tracts are continuous with the fibers that have once crossed in the pyramidal decussation and are formed by fibers which pass back again to the side from which they came. According to Sherrington, they are probably due to branching of the crossed fibers. Fibers thus branching are designated as "geminal fibers," and their existence is supported by the fact that fibers are frequently found in pairs in the same stage of degeneration; that two axis cylinders are found in one sheath, and that the geminal fibers are most abundant in the crossed tract.

There are other results given, one of which is that "interhemispheral degenerations tend to scatter and do not pass between identical areas of the two hemispheres."

Vergleichend-entwickelungsgeschichtliche und anatomische Studien im Bereiche des Centralnervensystems. II. Ueber die Fortsetzung der hinteren Rückenmarkswurzeln zum Gehirn. Dr. L. Edinger. Anatomischer Anzeiger, No. 4, 1889. Rev. in Neurolog. Centralbl, by P. Kronthal.

As the result of his own investigations on the spinal cord of the frog, Edinger has determined that voluminous fiber bundles arise from the posterior cornua and pass ventrad, crossing with those of the opposite side, both ventrad and dorsad of the central canal. After decussation these fibers pass cephalad for the most part in the anterior and to a small extent in the lateral columns. This crossing occurs in mammals also, but in man has escaped observation because it is inconspicuous in frontal sections. The path of the sensory nerves is, therefore: peripheral end organ, nerve, cell of the spinal ganglion, posterior root, union with a second nucleus, decussation, continuation to the lemniscus, whither the antero-lateral columns pass. The view that all the sensory fibers do not run in the posterior columns is supported by observations in tabes as well as by physiological experiments.

Progressive neurotische Muskelatrophie. Dr. J. Hoffmann. XIV Wanderversammlung südwestdeutscher Neurologen und Irrenärzte, Mai, 1889. Abstract in Neurolog. Centralbl, No. 13, 1889, by Dr. L. Laquer.

To the disease described by him under this name Hoffmann adds some anatomical facts. He cites several cases where the anatomical changes have been recorded and concludes that the following are made out: centripetal degeneration of the motor and sensory peripheral nerves, similar degeneration of the anterior and posterior spinal nerve roots, degeneration of the posterior columns in the lumbar region—from there cephalad only the columns of Goll are involved—, atrophy and disappearance of the multipolar ganglion cells of the anterior conua, with changes in the muscles as elsewhere described. Neither amyotrophic lateral sclerosis nor the various forms of poliomyelitis anterior nor Friedrich's disease (hereditary ataxia) nor ependymal sclerosis nor multiple neuritis produce such changes. From the lesion of tabes dorsalis it is distinguished by the immunity of the columns of Burdach above the lumbar region. (In a case quoted from Gombault and Mallet it is stated that the degeneration of all the fibers connected with the posterior root ganglion takes place while the ganglion cells remain normal! Rev.)

Anatomische Untersuchung eines Falles von amyotrophischer Lateralsklerosis. Dr. Otto Dornblüth. Originalmittheilung, Neurolog. Centralbl. No. 13, 1889.

This interesting case is given with some detail. At the end of the article there is a summary from which we take the following:

In a woman of 58 years with hereditary taint, and who had suffered from folie circulaire for 4 years, and exhibited increased muscular irritability for some time, there suddenly appeared paralysis of the upper extremities which soon decreased. After two years there were contractures in both legs. There were peculiar rises in the temperature, lasting a short time, immobile pupils and general atrophy. After two years more contractures in both arms; decubitus, death. The autopsy showed the lesion for amyotrophic lateral sclerosis. The degeneration in the pyramidal tract could be followed from the lumbar region to the level of the corpora quadrigemina at which the process was the more recent. The motor cells of the cord were degenerated, also the nuclei of the hypoglossus, anterior vagus and glossopharyngeus, facialis, and motor trigeminus. Moreover there was hyperæmia of the gray matter along the floor of the medulla and of the pons, fresh extraversations specially in the region of the posterior vagus nucleus (sensory) an old extraversation cephalad of the facialis knee, with secondary ascending atrophy of the right posterior longitudinal bundle. This case agrees with those previously reported in showing motor ganglia and tracts to be alone involved by the disease. The degeneration of the pyramids disappeared caudad of the internal capsule. (The brain was not examined microscopically, but showed no lesion at the autopsy). It is suggested that the case may illustrate Westphal's generalization that fiber systems which acquire their medullary sheaths the latest are those which most easily undergo involution, or that some toxic substance exercises a selective action on motor cells and fibers. The variations in temperature may have been connected with the extraversations into the sensory nucleus of the vagus.

Recherches sur la structure des cordons postérieurs de la moelle épinière de l'homme. N. Popoff de Varsovie. Archives de Neurologie, Mars, 1889. Rev. in Neurolog. Centralbl., No. 16, 1889, by Nonne.

Testing Becterew's statements on the components of the column of Goll, Popoff finds by way of confirmation that the group of fibers nearest the middle line becomes medullated earlier than the more lateral portion. The difference is clearer in the cervical than in the dorsal region. According to Popoff the mesial portion originates from the column of Clarke, the lateral from the posterior commissure.

A gerinczvelői idegek hátulsó gyökereiről. M. Lenhossék. Ung. Akademie, Sitzung v. 20 Mai, Auszug aus Orvosi Hetilap 1889, p. 21. Rev. in Neurolog. Centralbl, No. 16, 1889 by Jendrássik.

The course of the posterior spinal roots was studied by means of comparative anatomy and embryology. The entering fibers divide into three groups. A mesial (*innere*), intermediate (*mediale*), and lateral portion. The two former acquire their medullary sheaths earlier and are composed of larger fibers than the last. The mesial group passes through the substantia gelatinosa in a number of thick bundles, and bends to form the longitudinal bundles of the posterior cornua (Kölliker.) From here, after a longer or shorter course, they pass ventrad for the most part in the direction of the lateral cells of the anterior cornua, smaller portions to the lateral column and posterior commissure. This last group is small in man but more developed in the cord of the guinea-pig.

Of the intermediate group, a portion enters the column of Burdach and, after a longer or shorter course there, passes ventrad to the gray substance. Another portion passes directly through the substantia gelantinosa, ending in a network in the lateral cells of the anterior cornua; another portion to the column of Clarke and the posterior commissure.

The lateral group first passes longitudinally as the marginal zone of Lissauer, then ventrad through the zona spongiosa. The columns of Burdach consist of fibers from the intermediate group. The longer fibers pass from the lumbar roots to the cells of Clarke in the dorsal cord and a portion of the fibers pass from the cervical roots caudad to the same cell group, the major portion however passes to the nucleus of this column in the medulla. The Posterior Commissure contains: 1. fibers from the mesial and intermediate groups, medullated in embryos only 36 cm. long; 2. fibers arising from cells in the galatinous substance of Rolando becoming medullated after birth. One difference between these results and those of other authors is that Lenhossék traces none of the fibers from the posterior roots into the anterior commissure.

Zur Frage über Veränderungen der Nervencentren bei peripherischen Reizen. S. SSADOWSKI. Dissertation, St. Petersburg, 1889. Russisch. Rev. in Neurolog. Centralbl, No. 15, 1889, by P. Rosenbach.

The question to be solved was whether pathological changes could be induced in a ganglion by the stimulation of its peripheral nerve. The experiments were made on dogs and rabbits. Ssadowski stimulated on one side of the body, the nervus ischiadicus, vagus, auricularis magnus, or intercostalis. The stimulus was either by faradization or ligature of the nerves. In the first case the stimulus was applied for fifteen min. daily through several weeks. In the last the ligature was applied for a period of from 7—70 days. The animals were then killed and a microscopic examination made of the nerve and the associated ganglion. The nerve trunk in the neighborhood of the ligature showed evidence of degeneration; that which had been electrically stimulated did not. On the other hand, the ganglion in both cases showed atrophic degenerative changes (consisting in vacuolization, coagulation necrosis, and shrinkage) of the nerve cells, and at times infiltration with lymphoid elements and distention of the capillaries. These results are explained by the author as degeneration following disturbance of nutrition which was in time caused by excessive stimulation.

Further observations on the histology and function of the mammalian sympathetic ganglia. W. HALE WHITE, M. D. Journ. of Physiology, Vol. X, No. 5, July, 1889.

The study of the superior cervical ganglia in the human adult and comparison of it with other forms led White to conclude some time since that in man at maturity these ganglia were functionless. (See abstract AM. JOUR. PSY. Vol. 1. p. 329.) In the present paper he concludes from a study of the same ganglia taken according to the age of the subject that there is a progressive degeneration of the cells from birth on. He has further examined the semilunar ganglia in a similar way and finds the same general relations all around that were determined for the cervical. In examining the thoracic ganglia they are found much more constant in size and in general more normal, or at least never so degenerate as the first two that were studied. He concludes as the result of his investigation up to this point: firstly, that in lower mammals and young human beings the collateral ganglia (if we may judge from the superior cervical and semilunar) are functionally active, but that in monkeys there are evidences of commencing loss of function, which has completely disappeared in the human adult; secondly, that in man the function of the lateral ganglia is maintained well into adult life and only begins to disappear in old age. It is a curious fact that in all these cases the sympathetic nerves are described as normal. The possibility of degenerate ganglia associated with normal nerves in the sympathetic system is not explained by any existing view of the relation between cells and fibers in that region and at first sight, at least, is one of the most striking results.

On the Minute Anatomy of the Vagus nerve in Selachians, with Remarks on the Segmental Value of the Cranial nerves. THOMAS W. SHORE. Journal of Anatomy and Physiology, Vol. XXIII, pp. 428—451. Plates XX—XXI.

In a former paper (noticed in this JOURNAL Vol. II, p. 309) the author gave a summary of our present knowledge of the anatomy and development of the vagus in Petromyzon, Elasmobranchs, Rana and Amniota. The present paper contains the results of the author's researches upon the microscopic anatomy of the vagus of the skate (*Raia batis* and *R. clav.*). The nerve cells of the vagus of the skate are arranged in five groups. The nerve does not contain any non-ganglionated somatic motor fibers, and there is only one small faciculus of ganglionated somatic sensory fibers, viz., the small dorsal branch. The splanchnic motor and probably splanchnic sensory fibers are well marked, and are, as in the case of a typical spinal nerve, divisible into a non-ganglionated portion, which runs chiefly in the post-branchial branches, and a small-fibred ganglionated part, which is found in the branchials and visceralis. The vagus nerve of the skate, therefore, does not contain all the elements of a single perfect spinal nerve-metamer. It contains the typical elements of the so called sympathetic system, namely, splanchnic small medullated fibers some of which join a proximal set of ganglia, others passing on to a distal set. The proximal set of ganglia are represented by the branchial and viceralis ganglia, the distal set by the pre-branchial ganglia of the skate's vagus. F. T.

A demonstration of centres of ideation in the brain from observation and experiment. BERNARD HOLLANDER. Reprinted from the Journal of the Anthropological Institute, (London,) August, 1889.

The author attempts to correlate the modern experiments of the brain physiologists with the older observations of the phrenologists. Some half dozen "organs" are thus identified with the "centres" on the general principle that the "organ" is located in the region, where stimulation of the cortex gives rise to movements, gestures or facial motions that are expressive of the feeling for which the organ stands. The method pursued in correlating the two is however unscientific. Judging by the "discussion" at the end of the paper it was nevertheless received without any severe criticism. A paper of the same import was read by the author before the Anthropological Section of the British Association for the Advancement of Science.

Heat Centers in Man. ISAAC OTT, M. D. Brain, January, 1889.

From the study of the lower animals the author recognizes six centers, injury to which causes increased temperature. These are described as the cruciate, about the Rolandic fissure, the Sylvian at the junction of the supra and post-Sylvian fissures, the caudate nucleus, the tissues about the corpus striatum and the optic thalamus near the median line, and the anterior end of the optic thalamus itself. A few cases are presented which are considered to indicate the existence of similar heat centers in man, and in the closing sentence the task of localization with the required precision is said to devolve upon neuropathologists.

II.—EXPERIMENTAL.

COLORED SHADOWS.[1]

Whenever an object is illuminated from two different directions by lights which are approximately equal in intensity but more or less different in color, the shadows thrown by the object, especially if they fall on a white ground, will be colored. The difference in shade may be so slight that we ordinarily regard both lights as white, as in case of candle and moonlight, yet the colored shadows will appear. When the light from one source is white, its shadow is of the color of the other light; for it is illuminated by the latter light alone. But the other shadow, although it is illuminated only by white light, yet always appears of a color complementary to that of the neighboring field, which is illuminated by both lights together.

These shadows may be obtained in various ways, and they are of by no means infrequent occurrence. We see them in the theatre when colored lights are used, or in our room if we have a colored shade for our lamp. We notice them sometimes on the page we are reading, when the window shade is partly drawn, so that the daylight enters partly uncolored through the window, and partly slightly tinted through the shade. Goethe and others speak of beautiful effects caused by the setting sun shining on snow-covered fields. For experimental purposes the best plan is to use a dark room arranged in a manner soon to be described.

The color of the complementary shadow is at first sight hard to account for, and has given rise to much discussion. Helmholtz and others think that it is an effect of contrast with the neighboring field, due to a "deception of judgment," and that colored shadows form an *experimentum crucis*[2] of the psychological nature of color-contrast. There are certain facts connected with the shadows, they maintain, which entirely overthrow the physiological theories. We hope to show that in this they are wrong; that all the phenomena presented by these shadows can be explained at least equally as well —many of them, it seems, very much better—by a purely physiological theory.

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[1] Written for the Graduate Course in Psychology at Harvard University.

[2] Von Kries, "Gesichtsempfindungen," p. 131f

In the first place, the color of the shadow is evidently in some way due to the presence of the colored light which surrounds it; for it is always complementary to the latter, and if no colored light is present, the shadow appears grey. Yet in every case it is illuminated by white light only, and hence objectively does not change in color.

Its color then must be due either to retinal fatigue or to contrast of color. Helmholtz claims that it is primarily and especially the latter; for the color is seen at once if one takes care to regard the shadow without having previously looked at the surrounding field, or allowed the colored light to affect the retina. Its color is intensified, however, by aid of fatigue, if the eye is allowed to wander about between the shadow and the colored field. The facts which he and others rely upon to prove conclusively that this contrast is due to psychological, and not to physical or physiological causes, are described by him as follows. His shadows are produced by using candle-light, which is reddish yellow in color, and weakened day-light. They are therefore colored blue.

"Take a tube, blackened inside, and place it so that when one looks through it the eye sees only parts of the paper which lie in the shadow of the candle-light. Let at first only day-light fall upon the field, then look through the tube, and then allow the candle-light to enter. One sees now nothing of the parts illuminated by the candle-light, does not notice its presence, and the appearance of the portion of the paper which he sees through the tube remains unchanged. It follows that objectively the color of the paper in the shadow of the candle-light is unchanged.

"If however the observer directs the black tube through which he is looking so that a portion of the field which is being observed is lighted by the reddish-yellow light of the candle, then the shadow of the candle-light becomes blue. When the blue has very intensely developed itself, turn the tube again so that nothing but the subjective blue is in the field of view. Now the blue remains whether we allow the candle-light access to the rest of the paper or cut it off entirely,—which of the two we do is indifferent to the observer, since under these circumstances he sees nothing of the part of the field which is illuminated by the candle, and he cannot know whether its light is present or not. The blue color in such a case is so persistent that Osann decided it was objective. This assumption is easily disproved, in that the blue color still remains when the candle-light is extinguished. In the moment however when one removes the black tube from the eye, the subjective blue disappears, since one recognizes it as identical with the white which fills the rest of the field of view. No observation shows more strikingly and plainly the influence of the judgment on our color sensations. After the judgment is once fixed in consequence of the contrast, whether it be successive or simultaneous, that the color in the shadow of the candle-light is blue, the color still remains apparently blue even when the circumstances which have led to this decision are removed; until by removing the tube we make possible a new comparison with other colors, and through new facts allow our judgment to be otherwise determined."[1]

The nature of this "deception of judgment," which leads us wrongly to think the shadow colored, is concisely described by

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[1] Helmholtz, "Physiologische Optik," p. 394f.

Delbœuf in speaking of these experiments. The experiment proves, he thinks, "that the sensation of color can depend upon an act of thought alone."[1] If we look through a pane of red glass, rays which come through it to the eye from a white object will be red. Rays coming from a green object, if the conditions were perfect, would be entirely cut off, and the object would appear black; but the red glass always lets through more or less white light, and with every green is mixed some white; so that some grey rays would come to the eye from a green object. Nevertheless we do not judge the white object to be red, nor the green to be grey, but they appear to us more or less in their natural colors. This is because we are accustomed to judge of the colors of objects through varying modifications of the light which surrounds us, and which changes continually according to the state of the atmosphere, the aspect and disposition of the clouds, the color of reflecting objects, etc. Hence we have learned to judge correctly the color of objects in making allowance for the color of the medium through which we look. So in the case of the colored shadow, which in reality is grey, we judge it to be blue—if we are using Helmholtz' method for producing them—because we think that we see it through a reddish-yellow medium. "When we have once judged it blue, if we make use of our tube, there is no reason for us to change our decision; nor is there any more reason to change it if the colored light is removed, since we observe no change of condition. But when colored light and tube are both removed, we judge at once that the shadow is grey, because we believe that we see it through a white medium; and when we replace our tube before the eye we continue to judge it grey, whatever change is made in the color of the light, because again we observe no variation of condition such as would cause us to modify our judgment. It is then proved, we believe, that the judgment of color rests not only upon a special property of the visual sensorial substance, but also upon unconscious anterior judgments, which have become so through habit, or because they depend upon instinct."[2]

Now let us examine these experiments more closely, and see if we cannot explain the facts quite as well by a purely physiological theory. For these experiments the following arrangements yield the most satisfactory results. In the shutter of a dark room is an opening, which, being furnished with a pane of colored glass, allows only colored light to enter. At this opening is fixed a small shutter, so arranged that it can be closed by means of a cord reaching to the table on which the shadows are cast, and can open itself automatically when the cord is released. White light is obtained through an opening some distance removed from the first, or from a gas jet or lamp. An opaque object on the table throws two broad shadows on to a white ground. Suppose now that we place a pane of green glass at the opening. The shadow cast by the white light will be strongly illuminated by the green light, and will be of that color. The general ground, illuminated by both lights equally, will be of a whitish green. The second shadow will appear to be of a color complimentary to this, a strong red, although it is illuminated only by white light. It is with this latter shadow alone that we are concerned.

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[1] Delbœuf, "La Psychologie comme Science naturelle," p. 58.

[2] Ibid. p. 61ff.

First let us see if, as Helmholtz says, we have to do with contrast effects only, and not with retinal fatigue. For this purpose we arrange our apparatus with the green glass at the opening, and have upon the table a tube so fixed to an iron standard that its upper end is stationary, while its lower end can be moved about at will. Let the shadow fall upon the paper, and mark a point within it at a little distance from its margin. Then arrange the tube in such a way that by looking through it you can see a portion of the shadow, including the point marked and also a portion of the colored field adjoining. Now by means of the cord close the small shutter, thus excluding the green light, rest the eye thoroughly, and then apply it to the tube. Carefully fixate the point marked, keeping the eye entirely from wandering, and open the shutter. A portion of the field visible will at once appear green, from the green light falling upon it. The shadow will appear of a slightly red tinge, with a tendency to decrease in intensity. This red is evidently purely an effect of contrast with the green, if the eye has been kept rigidly fixed. If the tube is now so moved as to cover the shadow only, and a new point is at once fixated, the red does not last for an instant on that portion of the retina on which the image of the shadow had fallen, but rather gives place to green or grey; while the other half of the retina, on which green light alone has worked, does experience the sensation of red, for it has been exposed to green-fatigue. This result follows clearly only under the most favorable circumstances. The difficulty of steady fixation, and of renewing the fixation after moving the tube, make the experiment difficult of success. The result varies somewhat also according to the length of time of fixation, to the size of the tube, to the amount of diffused light reflected from its sides, to the strength of the colored light and vividness of the shadow, to the care with which the eye is kept wide open, etc. Of course the various phenomena which accompany steady fixation, —the periodical disappearance and reappearance of the field, etc.,— do not enter into consideration. But one result is always certain, if the experiment be performed with care,—the red of the shadow does not persist. The following modification of this experiment is perhaps less liable to failure on account of the difficulties mentioned above. After fixating the marked point in the red shadow close the shutter, thus excluding the green light, without moving the tube. Now the green almost surely gives place to red, and the red to a faint green.

This experiment gives us a very important result: the red color of the shadow, which Helmholtz found so lasting even when the green light was shut off, is not purely a contrast red; for the pure contrast red, as we have just seen, always disappears at once when the contrasting field is removed. The red which persists when the green light is removed is only the red induced when the eye has been allowed to wander over the field, or when it has been already somewhat exposed to the green light pervading the room, and when therefore the color is due not to contrast alone, but also to the effect of green-fatigue.

All these phenomena admit of an easy physiological explanation, but directly contradict the psychological claim, namely, that the red of the shadow, even when due to contrast alone, persists when the neighboring green field is excluded. Let us, for example, consider the explanation according to Hering's physiological theory. He maintains that the excitation of any portion of the retina induces a stimulation of the neighboring parts, tending to produce in them the sensation of the complementary color; this being the physiological cause of simultaneous contrast. This would account for the color of the shadow in the original fixation. The continued action of the green on one portion of the retina would produce green-fatigue, by diminishing the green-red visual substance (or by building it up above the normal, according as green is the dissimilative or the assimilative color.) The red process on the other portion of the retina would produce the opposite effect, giving rise to a slight red-fatigue. On suppressing the external color-stimulus, (by closing the shutter or by moving the tube,) each portion of the retina tends to return to its normal condition, with the effect that the green yields to red and the red to green.

Having now learned that the red when it persists cannot be merely a contrast red, but must be an effect of green-fatigue, perhaps we can explain the phenomena which Helmholtz describes, in accordance with the physiological theory. Helmholtz' first experiment, which we quoted above, we make as follows: Let first the shutter be closed, excluding green light, and let the movable tube be so fixed that it is directed upon the subsequent position of the shadow, without including any of the neighboring field. Look now with one eye through the tube, the other being closed. The ground will appear grey. If now, with the eye still at the tube, the shutter be opened, causing the shadow, no change of color will be observed, but merely a slight one of illumination, or perhaps a somewhat greenish tint due to diffused light. This latter will yield to a faint negative (fatigue-induced) red on closing the shutter, lasting for a moment only. The shadow under these circumstances naturally does not appear red; for the physiological cause of the redness is not present, in that there has been no exposure of the retina to the green light of the field.

To one not looking through the tube, however, when the shutter is opened, the shadow at once appears red and the ground a whitish green, which rapidly becomes weakened in intensity. The latter fact is due to the diminished sensitiveness to green rays, on account of retinal fatigue; or, in Hering's language, the exposure of the retina to green light rapidly modifies the green-red visual substance, diminishing the sensitiveness to green and increasing the sensitiveness to red; the stimulus from the green rays remaining constant, the weight of the green element in the total sensation rapidly tends to decrease, of the red element to increase; hence results as total sensation a much lighter green. As the green becomes less intense, the red of the shadow becomes more so, for the same reason.

If now we look again through the tube, first at the shadow with a small portion of the general field at its edge, then at the shadow only, the shadow continues to appear red. For the retina, moving about over the green field, has suffered green-fatigue. Therefore when the green stimuli cease, the tendency is to restore the green-red substance to its normal condition, and the red process results. This condition now persists, even when the shutter is closed, for the color is due not to the objective color of the shadow, nor to the judgment, but to the physiological condition of the retina; and it will persist until equilibrium is restored. That this restoration of equilibrium does gradually take place is a fact overlooked by Helmholtz; but we can easily convince ourselves of it by experiment. After directing the tube onto the shadow alone, with the shutter open or closed, we observe by patient watching that the red color slowly disappears until finally the field appears white or grey. Von Kries, who supports the psychological explanation, yet acknowledges that the "color does not last indefinitely long, but soon becomes doubtful."[1] The same fact has been observed by several persons who have performed the experiment with me, although they had no suggestion of what would probably occur. This is exactly the result to be expected if we accept the physiological explanation, as we have seen. But from the psychological standpoint it is hard to see why the red color should disappear at all, so long as we continue to look through the tube. For, as Delbœuf says in the passage quoted above, there is no reason to change our decision, if the color is due to an "unconscious anterior judgment," for the observable conditions do not change. We have therefore already strong reasons to doubt the assertion of Helmholtz that the red color is one which persists "after the circumstances which caused it have disappeared," owing to continued "deception of judgment."

The length of time during which the red color persists varies greatly, and sometimes the disappearance is very slow. It seems to bear some relation to the strength of the green light, and the length of time that the eye has been exposed to it. If, while the red still persists, one lay aside the tube and glance at the field, (the shutter being closed or the colored glass removed,) the color at once disappears; for it is thrust "under the threshold" by the stronger sensations coming from the field. But one can often see it again when these stronger sensations cease on applying the eye again to the tube. If one continue however to gaze through the tube alone after obtaining the red color, there are no neighboring active fields to influence it and no rival processes to interfere, and the change is sometimes very slow. A subsequent experiment will illustrate another case where under similar conditions an after-image is long retained, showing that the slowness with which the equilibrium is restored in this experiment is by no means exceptional.

When the sensation of red has become weak, there occurs a doubtful stage, where the phenomena are liable to vary in different observers. For example, while still watching at the tube with the shutter closed, if the other (unfatigued) eye look through a second tube at the white ground, and its field be compared with that of the observing eye, the red of the latter will sometimes seem to disappear suddenly, and the field is seen to be white. Von Kries speaks also of being able, at this stage (where the process has become very weak), to hasten or delay the change, and see the color almost at will red or white. This appears to belong to that class of cases where, the sensation being weak, it is often doubtful whether it exists or not. We often cannot tell whether we hear or imagine a faint sound, and sometimes under favorable circumstances we think that a musical note still continues, even after it has ceased. So here, at this doubtful stage, the sensation may be imagined for a short time after it ceases, and then disappear on comparing it with the white field. This says nothing however against the original red having been due to actual physiological processes. Quite as often, when one thinks the red color entirely gone, it will again be seen if the field seen through the original tube be compared with the grey field through the second tube. Again, after long gazing through the tube, there is always a darkening of the field underneath, due to fatigue of the black-white substance, and at its edges more or less contrast with the black sides of the tube. This effect is liable sometimes to be taken for a weak sensation of color.

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[1] Gesichtsempfindungen, p. 128.

These are all cases belonging to a large class of judgments where very weak nervous processes answering to sensations and nervous processes answering to pure mental images, are indistinguishable from one another, and where weak impressions are easily misinterpreted.[1] But they have not necessarily anything to do with color-contrast and with the cause of the shadows which we are considering.

We have thus reviewed all the "crucial" experiments of Helmholtz, and found them valueless as proofs of the psychological nature of color-contrast. A few further experiments are of interest, and easily admit of a physiological explanation. For instance, if, laying aside the tube, we steadily fixate for a considerable time one point of the middle of the shadow, the red becomes weaker and finally almost disappears, leaving the shadow greyish except at the edges, where the contrast is more marked. This is because the fixation is favorable to the restoration of the equilibrium disturbed by green-fatigue. If again we fixate the shadow, and then after a considerable time shut off the green light, the rest of the retina is subject to a bright red sensation, due to the restoration of the exhausted green-red substance; and the portion corresponding to the shadow experiences the sensation of green, due largely to contrast with the red.

Finally, the following experiment, to which we have already referred, illustrates the slow disappearance of an after-image under favorable circumstances, and thus shows that the same phenomenon in our tube experiment is not exceptional. Admit only white light to the room, then look for a time through the tube at a colored field (e. g., a piece of red paper); then move the tube so as to cover a white or grey ground, or remove the colored paper. The color complementary to the original field (in this case green) will be the color now seen, and not the original color (red), nor white or grey. And this color remains visible for a long period of time, as the red did in the shadow experiment. In the shadow experiment, after gazing at an apparently colored field (the shadow), the same color continues to be seen in the tube on a gray ground; in this experiment the complementary color. But the same law operates in both cases; for the two colored fields observed are due to different causes. In the first, the red is due, not to objective, but to subjective stimuli. The continuance of this red produces no red-exhaustion, but the gradually disappearing green-exhaustion merely continues, and is present so long as the red color continues as its sign. In this second experiment the color is objective, and produces exhaustion of that color, leading to a restoration of equilibrium which produces the sensation of its complementary color.

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[1] On these cases, Prof. Sully, "Illusions," (Internat. Sci. Series). p. 55: "It is a law of sensory stimulation that an impression persists for an appreciable time after the cessation of the action of the stimulus. This 'after-sensation' will clearly lead to illusion, in so far as we tend to think of the stimulus as still at work."—p. 41: "It is evident that all indistinct impressions are liable to be wrongly classed. Sensations answering to a given color or form are, when faint, easily confused with other sensations, and so an opening occurs for illusion."—p. 39: "It is possible for the quality of an impression, as, for example, of a sensation of color, to be appreciably modified when there is a strong tendency to regard it in one particular way."

By the above experiments we have endeavored to prove the invalidity of the claims that colored shadows must necessarily be explained as an effect of deception of judgment, and that they are therefore a proof of the psychological nature of color contrast. We leave it still an open question whether contrast is psychological or physiological in its nature. Hering's formulæ seem to apply to it as well as do those of Helmholtz. Colored shadows throw no light on the controversy; for the experiments with colored shadows, which have been supposed to prove conclusively the psychological theory, are experiments not in color-contrast but in retinal fatigue. The unwarranted claims of the "psychologists" have resulted from the fact that hitherto care has not been taken to distinguish between the two phases of the shadow—that of pure contrast, which ordinarily is never seen, and that due to fatigue by the prevailing light, which begins at once over the entire retina, since the eye keeps moving continually over the field. Helmholtz waited for this effect before performing his tube experiments, as we notice by his direction to allow the blue of the shadow first to become intensely developed; and then he wrongly called the result an effect of contrast. His followers have done the same. Our positive result, besides the separation of the two phases of the shadow, is this: We have seen that the color of the shadow persists in the experiments with the tube only when it is due to retinal fatigue; that its long continuance is paralleled by an experiment in which the persisting color is an after-image which is not caused by "unconscious anterior judgments" induced by the belief that we gaze through colored media; that the color gradually disappears, a fact which cannot be accounted for by the psychological theory; that when it disappears suddenly by comparison with the grey field outside, it is because the sensation is thrust under the threshold, or has reached the stage where it is more imagined than seen; and that all the other phenomena observed are entirely compatible with a physiological explanation—some of them being thus even better accounted for. These facts, we believe, entirely disprove the claim that only a psychological explanation of colored shadows is possible, and make some physiological explanation at least possible—perhaps even absolutely necessary.

EDMUND B. DELABARRE.

Ueber den Grund der Abweichungen von dem Weber'schen Gesetz bei Lichtempfindungen. H. EBBINGHAUS. Archiv f. d. ges. Phys. Bd. XLV.

The researches of König and Brodhun, more rigid than any that have hitherto been made (this JOURNAL, Vol. II, p. 330), as to the degree of exactness with which Weber's Law applies to sensations of light-intensity, over a wide range of variation of intensity, and for six different points of the color scale, have induced Ebbinghaus to endeavor to account for the fact that departure from the law is the rule, and that its exact applicability holds only over a small range of medium intensities. Without attempting any explanation of the fact that the intensity of a sensation increases as the logarithm of the intensity of its objective cause, he endeavors to show why that should *not* be the case at the two ends of the intensity-scale, and in the following way. Assuming that the process which goes on in the retina is of a chemical nature, it is necessary to apply to it the conceptions that are current in modern chemical theory. The chemist has given up the idea with which he started out, of the absolute uniformity of the substances with which he deals. As the physicist now identifies the pressure of a gas confined within a given volume with the *average* energy of motion of particles which are flying about with countless different velocities, so the chemist is now forced to look upon a given chemical constitution, not as a fixed and homogeneous, but as a *mean* state of aggregation of a given lot of atoms or groups of atoms. (B. Horstmann, *Theoretische Chemie*). When any agent is applied whose effect is to produce increased dissociation, it will have a fixed *average* effect upon the whole mass of matter concerned, but its actual effect upon any individual molecule will depend upon the condition of that molecule before the new force was applied. If it was already so *loose* in structure (to use a word for which there seems to be an absolute necessity), that is, if the atoms which composed it were so remote from their mean center and performing such rapid oscillations of their own that the force applied was just sufficient (or more than sufficient) to break its bonds altogether, then that molecule would be destroyed. A molecule which was of firmer constitution would not be destroyed by that particular application of energy, but would simply be brought nearer to a state of dissolution than it was before. If, now, the disgregation conditions of the different molecules occurred in accordance with the usual law for distribution about a mean, much the greatest number of the molecules would be in a state of disgregation near the mean, a gradually diminishing number would be of looser, and also a gradually diminishing number of a less loose constitution. Consequently, the effectiveness of any applied force (the light falling upon the retina in the case before us) would be greatest if it were just capable of breaking up the molecules near the mean. If it were of a less intensity, there would be a relatively smaller number of molecules which it was capable of destroying; if its intensity were increased, there would again be a relatively smaller number of molecules left to be destroyed. This agrees in sense with the departures from Weber's Law; a low intensity and a high intensity of light are both less effective than that law would require them to be. Ebbinghaus attempts to show that there is a very exact correspondence between the departures exhibited by König and Brodhun, and those required by his theory. But in determining the actual distribution of disgregation states among the molecules, he transforms the kinetic energy of each atom into potential energy and then counts up the influences which affect, not its actual kinetic energy but its equivalent energy of position. This latter proceeding is plainly unjustifiable. The coincidence which he obtains between theory and fact seems to be, nevertheless, remarkably close.

It is a very curious proceeding in scientific method to attempt to account for the deviations from a law, when there is absolutely no explanation of the law itself—no physical explanation, that is; Mach's explanation is of a teleological nature. It seems to us to be, however, in the present instance, a proceeding which is not wholly illegitimate. The explanation itself rests, of course, upon the assumption that the relativity of Weber's Law is an objective fact, and not an affair of the judgment. C. L. F.

Beiträge zur experimentellen Psychologie. HUGO MÜNSTERBERG. Heft 1. Freiburg (J. C. B. Mohr) 1889. 8vo. pp. 188. 4 mks.

Dr. Münsterberg, a docent in philosophy at the University of Freiburg, announces a series of publications (of which this is the first installment) similar to the studies from Wundt's laboratory, with the exception that Dr. Münsterberg personally takes part in all the experiments and himself writes up the results. He hopes to publish about three numbers a year, and the new adventure certainly deserves a hearty encouragement, especially considering that the laboratory in which the studies are made is a private one without governmental endowment. It may as well be said first as last that the chief defects of these contributions (a defect perhaps equally prominent in Wundt's *Studien*) is the undue prolixity of the treatment, that so buries the real objects of worth as to give almost as much credit to the one who digs successfully into the heap as to him to whom it owes its origin. The complete results obtained by Dr. Münsterberg could have been forcibly stated in a maximum of seventy-five pages, with at the least doubling the number of his readers, and increasing ten-fold the appreciation of his work. It would perhaps be irrelevant to emphasize this point were it not that psychology has suffered from this weakness so severely in the past and will not gain its proper place amongst the sciences until it gives its results a truly scientific form.

Of the two studies here presented the first states the author's position on the general relations of body and mind, while the second gives an account of some ingenious experiments upon the time-relations of mental phenomena under the misleading title of "concious and unconcious combinations of perceptions (*Vorstellungsverbindungen*)." The former is a somewhat complicated statement of the modern "psychophysic" view of the mutual influence of physical and psychical conditions with special reference to the factors of apperception and consciousness in the process. The train of thought is much broken into by digression (particularly of a controversial nature against Wundt) and is only indirectly concerned with the experiments, although the author tells us that his guiding *motif* was to throw light on the relation of the subject that watches and records mental operations to that other subject that in part with, and in part without consciousness elaborates them.

The experimental contributions fall into two parts, the facts brought out in them being the following:— (1) Following out the distinction established by Lange between a "sensory" mode of reaction (in which the attention is concentrated upon the expected stimulus) and the "motor" form (in which the attention is fixed upon the intended movement), Dr. Münsterberg measures the reaction-time to a sound with each of the five fingers with each form of reaction, and finds on the average a sensory time of 162σ and a motor time of 120σ ($\sigma = .001$ second), a difference of 42σ, considerably less than Lange's (100σ). (2) Next, the words one, two, three, four, five were called out irregularly, and the thumb, forefinger, etc., were to be raised when the corresponding number was called. Sensory time, 383σ; motor, 289σ. (3) The process remains the same, but the Latin declension *lupus, lupi, lupo, lupum, lupe,* is substituted for the number words, evidently a more artificial association. Sensory time, 465σ; motor, 355σ. (4) The movements of the five fingers are associated with the cases of the three following German pronouns, *ich, meiner, mir, mich, wir*; or *du, deiner, dir, dich, ihr*; or *der, des, dem, den,* *die*, the same reaction following to any one of three stimuli. Sensory time, 688 σ; motor, 430 σ. Errors, *i. e.*, raising the wrong finger practically never occurred up to this time; from here on they occur, but only in the motor reactions, and always consist in the raising of a neighboring finger. In the present case the errors form 10 per cent. of the reactions; the second finger being frequently raised to *du* apparently from its place in the series, *ich*, *du*, *er*. (5) The process becomes more general; if a noun is called, the thumb is to respond; if an adjective, the next finger; if a pronoun, the next; if a number, the next; and if a verb, the little finger. The words are all monosyllabic; no word occurs twice, and the general association of parts of speech with fingers is well learned in advance. Sensory time, 712 σ; motor, 432 σ; errors, 30 per cent. (6) The process remains the same, the categories being respectively, a city, a river, an animal, a plant, an element. Sensory time, 893 σ; motor, 432 σ; errors, 12 per cent. (7). The categories are an author, a musician, a naturalist, a philosopher, a statesman or general. Sensory time, 1122 σ; motor, 437 σ; errors, 25 per cent.

This remarkable and rapidly increasing difference between a sensory and a motor reaction demands a more complete explanation in the light of the facts just presented. After spending a needlessly long argument in showing that Wundt's apperception theory fails to explain the facts, Dr. Münsterberg concludes that in the sensory apperception we attend to one step at a time, allowing each factor to pass through the mind *seriatim*, (and thus sensory reactions remain a trustworthy test of mental complexity); but that in the motor times we have a distinct overlapping of mental processes, the mechanism being a short circuit between the finger-moving centers and the particular word called, the processes of recognizing the word as belonging to a certain category, and that category as associated with a certain finger not emerging in consciousness and being performed simultaneously with the other process. This process would be about the same in cases (4) to (7) and these give about the same motor, through vastly different sensory times. In other words, motor complexity is a different matter from sensory complexity, and a comparison of the gradual increase of each time from (1) to (7) will reveal many suggestive points, with reference to which every theory of association or perception must be built.

The second study is upon the association of ideas. There are two subjects, *M* and *R*, who react by pressing a key in pronouncing their reply-word, while Dr. Münsterberg had pressed the key in speaking the last syllable of the call-word or the call-sentence. Besides the times the average variation (*v*) is given.

(1) Simple repetition of the call-word occupies *M*, 403 σ (*v*. 60 σ) and *R*, 362 σ (*v*. 70 σ). (2) Ordinary association-time for *M*, 845 σ (*v*. 140 σ); *R* 948 σ (*v*. 170 σ). (3) A limited association, the general type being, "Given a general term to name an instance under it." The kind of relation was constantly varied, such as, "A German wine, Rudesheimer," "A Greek poet, Homer," etc. *M*, 970 σ (*v*. 200 σ); *R*, 1103 σ (*v*. 210 σ). (4) An association limited to a single answer, after the pattern of question and answer: *e. g.*, "On what river is Cologne? Rhine." "In what season is June? Summer." *M*, 808 σ (*v*. 180 σ); *R*, 889 σ (*v*. 140 σ). (5) The answer is now the result of comparison and judgment; e. g., "Which is larger, a lion or mouse?" "Who is greater, Homer or Kant?" *M*, 906 σ (*v*. 180 σ); *R*, 1079 σ (*v*. 220 σ). (6) Same as (5), but each query is preceded by a series

of about a dozen words of the same category as the terms compared. Thus: "Apples, pears, cherries, nuts, peaches, grapes, strawberries, dates, figs, raisins, — which do you like better, grapes or cherries? Cherries." *M*, 694 σ (*v.* 130 σ); *B*, 659 σ (*v.* 160 σ),—a shortening of 212 σ and 460 σ as compared with (5). (7) A combination of (3) and (6); instead of first asking for a drama of Goethe's, and then asking which is the finest, this, that or the other, we ask at once, "Which is the finest drama of Goethe?" *M*, 962 σ (*v.* 180) σ; *B*, 1137 σ (*v.* 160 σ). (8) This is a comparison between a given term and a term derived by process (7); thus, "Which is the more westerly, Berlin or the most important German river? Rhine." *M*, 1844 σ (*v.* 370 σ); *B*, 1866 σ (*v.* 340 σ). (9) Bears the same relation to (8) that (7) does to (4); the type of query being, "Which lies more westerly, Berlin or the river on which Cologne is situated?" *M*, 1291 σ (*v.* 180 σ); *B*, 1337 σ (*v.* 230) or 553 σ and 529 σ less than (8) while (7) is only 154 σ, 248 σ less than (4). (10) is (9) preceded by a series of terms of the same category as those to be compared. *M*, 1153 σ (*v.* 170); *B*, 1145 σ (*v.* 210), or 138 σ and 192 σ less than (9). The results are based upon 890 experiments in all.

After again fiercely combatting Wundt's apperception theory in its relations to these results, Dr. Münsterberg attempts their interpretation, in which the four following relations may be selected as most important: (*a*) that the time of a free association is shorter than of a limited association, (2) shorter than (3); (*b*) the question-answer association is shorter than the limited, (4) than (3); (*c*) that the reading of the series of words before the question shortens the time, (6) is shorter than (5) and (10) than (9); (*d*) the combination of any two or three factors in the same process takes less time than the sum of the times needed for each of the factors separately; compare (9) with (3) and (5). Relation (*a*) is not new and is readily explained by considering that in one case *any* association aroused by the call-word will answer, while in the other case several associations may arise that must be rejected. Fact (*b*) seems to indicate that we do not call up first the general notion and then the particular, but seem to have a direct and usually prominent association with the particular, the irrelevant general association not being "apperceived." The third fact indicates the mechanism of preparation, certain general lines of association being already ruled out by the formation of the category-series, and thus less of the mental labor comes into the measured time. The fourth circumstance shows again the overlapping of mental processes; the mind is not a point through which each process must pass in turn, but is a plane in which the most complex interactions have play. For the acceptance and further development of the interesting results of these studies we must look to the future. J. J.

L'Énergie et la Vitesse des Mouvements Volontaires. CH. FÉRÉ. Revue Philosophique. Juillet, 1889.

In this paper Dr. Féré gathers up the results of a number of brief studies, for the most part reported during the past year in the *Comptes rendus de la Société de Biologie.* Beginning with pathological subjects, he shows that the reaction-time of hysterical hemianæsthesics is generally longer than normal (and, as some of his experiments seem to show, the rate of nervous transmission on the diseased side slower) when the diseased side has part in the process, the lengthening corresponding in general to the diminution of sensibility and muscular power. These changes in force and rapidity depend upon differences in nutrition which are in part demonstrable. The plethysmograph shows an increase of the volume of blood in the hand preceding the voluntary reaction, apparently one of its prerequisites. Other experiments show that the electrical resistance is less, and that the reduction of oxyhemoglobin is slower upon the anæsthesic side. The reaction-times and the reduction of oxyhemoglobin are strongly influenced by sense stimulation and by emotion, either natural or hypnotically suggested. Experiments were also made upon epileptics with reference to the quickness of the response before and after the paroxysm, the last being slower and corresponding to the anæsthesias and weakness which seem, in some degree at least, always present. The slowing does not seem to depend on the frequency of the attacks; it serves, however, to show the persistence of the stupor. Here also slow reactions accompany diminution of oxyhemoglobin, a fact that the author uses in defending the fatigue theory of post-epileptic paralyses.

Experiments on normal subjects show the same general relations between muscular vigor and quickness of response. With a new dynamometer of varied applicability, Féré has been able to compare the powers of extension and flexion of the different fingers among themselves and with their reaction-times. The following table shows the results in a single case:—

	FLEXION.		EXTENSION.	
	Dynamometer (Kilograms.)	Reaction-times (Seconds.)	Dynamometer (Kilograms.)	Reaction-times (Seconds.)
Thumb	4.200	0.163	1.200	0.19
Index finger	4.000	0.191	1.000	0.261
Middle "	3.500	0.193	.900	0.28
Ring "	2.000	0.201	.600	0.299
Little "	1.900	0.203	.400	0.31

A similar table from a pianist shows quicker reactions and greater strength in the last three fingers. In right-handed subjects the reaction-time is a little quicker on the right than the left, even when the reaction is made to the same stimulus by both, (in which case both are a little slower): *e. g.* (flexion) right hand alone, 0.12 sec.; left hand alone, 0.16 sec.; both hands together, right 0.14, left 0.18. Some subjects reversed the last relation, and developed more force and reacted more quickly with both hands; they were, however, rather defective epileptics. Normally it would seem that the "rapidity and abundance of the nervous avalanche are by so much the greater as the ways of discharge are less numerous." In the lower extremities which act most powerfully in extension, the extensors give the shortest reaction-times. Normal subjects give quicker and more forcible propulsions of the tongue than deaf-mutes and stutterers. The effect of warming the hand in hot water is to quicken reaction, the greatest change appearing in the fingers reacting most slowly before. Féré's experiments make an impression of care and judgment as compared with those of Rémond, reviewed in a previous number (AM. JOUR. PSY., II, 486). They were, however, executed in part with the same questionable instrument, but in part also graphically with Marey's pneumatic drums. The number of observations going to a series were apparently small, and mean variations are never given—a serious omission if any intelligent judgment is to be passed upon the results.

Experimentelle Studien zur Individualpsychologie. AXEL OEHRN. Inaug. Dis. Dorpat, 1889.

Simplification is a thing much to be desired in psychic measurements, and until such can be secured, their application which might be considerable must remain comparatively limited. The point of Oehrn's thesis is the demonstration of the applicability of a few simple methods—not all new, but never before applied on quite this scale. His subjects counted letters from the printed page, added single syllabled digits to 100, wrote at dictation and read through successive periods of 5 minutes, and committed to memory numbers and meaningless syllables, the experiments generally lasting 2 hours and the subject working at the top of his speed. These processes, though more complex than those measured in reaction-time experiments, are simple enough to lead to interesting and rather regular results. The following table shows the average time on ten subjects in thousandths of a second for a single stage of the different tasks, *e. g.* counting one letter, reading one syllable, etc. :—

	Average Time in 0.001's Sec.	Max.	Min.	Average of Mean Variations in per cents.
Counting single letters,	406	530	317	4.2
" letters by threes,	323	440	209	5.0
Adding pairs of numbers,	1244	1583	754	4.6
Writing single letters,	435	603	331	2.6
Reading single syllables,	138	172	116	3.4

	Average Time in Seconds.	Max.	Min.	Average of Mean Variations in per cents.
Learning 12-place numbers,	9.6	20	4.2	14.7
" 12 nonsense syllables,	11.8	21.4	7.80	27.4

The author's analysis of these processes and the comparison of his results with those of Cattell, Berger, Dehio and others, must be consulted in the original. Of more importance, however, than these absolute results are the relative ones which bear upon attention and the effects of practice and fatigue. The former is studied in the mean variation which Buccola has aptly called "the dynamometer of attention." Reading and writing, for example (see table above,) are almost automatic in educated persons, while the learning of nonsense syllables requires the most vigorous voluntary attention. By comparing the amount of work accomplished in successive periods during the two hours the effects of practice and fatigue are discovered. The typical curve shows an ascending phase in which practice more than balances fatigue, quickly followed in general by a descending phase in which fatigue more than balances practice. From this there are not a few variations, mostly explicable by changes in the three factors (attention, practice and fatigue) from which the curve results. Fatigue announces itself by irregularities even before the summit of the curve, but these again disappear as the subject becomes too tired even to "spurt." The relative gain through practice is less and less as practice increases, as has before been shown by the experiments of Guicciardi and Cionini. The last chapter of the pamphlet is devoted to individual differences, and though several are clearly demonstrated, the number of persons studied was too small for generalization. Among other suggestive points may be mentioned the evidence for a 24-hour periodicity in capacity for intellectual work; of the two persons tested one was at his best in the evening, the other in the morning.

Versuche über die Ablenkung der Aufmerksamkeit. **ARVED BARTELS.** Inaug. Dis. Dorpat, 1889.

The writer of this thesis, like Oehrn, a pupil of Kræpelin's, has studied the distraction of the attention in the field of light sensations by finding the variation in the threshold stimulus for one eye, when its reception was preceded at a short interval by another stimulus in the other eye. For the description of the rather complex apparatus and the detail of the experimentation the reader must be referred to the original; the following points, however, may be mentioned: the threshold was fixed at the point where half the judgments were correct (method of right and wrong cases); the distracting stimulus was 1515 times as intense as that of the threshold, and lasted 0.171 sec. against 0.448 sec. for the latter; the intervals between the two were varied from 0.1 to 10 seconds, those from 1 to 6 receiving most attention. Evidence of distraction was found, lasting as long as 6 seconds, but evidence also of a diametrically opposite effect, the intended distraction acting like the warning signal in reaction-time experiments and helping to concentrate the attention. The intervals at which this was most marked were about $2\frac{3}{8}$ sec. and $4\frac{3}{4}$ sec. with some signs of another such period at a little more than 7 seconds. This rhythmic change recalls those found by time-sense experimenters, but is much longer (Estel 0.75 sec.; Mehner 1.4 sec.; Glass 1.25 sec.). It agrees, however, pretty well with the interval in estimating which the smallest error is made, and with this it is probably connected; L. Lange, Ewald and Wundt have found the two-second warning to be the most effective one in reaction-time experiments. The experiments tried show that there is no connection with the two-second periodicity of the unconstrained attention (N. Lange). When the interval between the stimuli varied each time and the subject only knew the limits within which it might vary, he seemed to adjust his attention to a point midway between.

Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte. G. E. **MÜLLER** und **FR. SCHUMANN.** Reprint from Pflüger's Archiv., Bd. XLV, pp. 37–112, 1889.

This very important paper is an investigation of the simple illusion which makes a light weight lifted after a heavy one seem disproportionately light, and *vice versa*. The importance for psychophysic experimentation of such a study is obvious, but the writers value their work less as a contribution to that subject than to the theory of motor adjustment and organic memory. The weights were lifted behind the back with every precaution to secure accurate results. A typical experiment is as follows: A standard weight of 676 grammes was compared five times each with weights of 626, 676, 726, 776, 826, 876 grammes. The last was recognized as heavier every time, the next-to-the-last four times. The standard weight was then compared 30 times with a weight of 2476 grammes, and after that once each with weights of 926, 876 and 826 grammes. All seemed lighter. After five more comparisons with the 2476 gramme weight, it was again compared with the last three, and so the experiment continued. In this case the standard and comparison weights were lifted with different hands, but the illusion occurs in single handed experiments also, and it is even possible in a certain degree to disturb the judgments of weights lifted with one hand by lifting heavy weights with the other.

This illusion is due, not to "contrast," but to an unconscious adaptation (*Einstellung*) of the motor centers to the more vigorous impulses required by the heavier weights. In the normal comparison of weights the nervous discharge for the lifting of each, though wholly unknown in amount to the subject, is the same. *The judgment is made from the speed with which the weight comes up.* When by the change of weights a discharge adapted to a heavy one meets a light one, the latter rises with an unaccustomed velocity and is therefore pronounced lighter. Such motor adaptations may be of different kinds, and, corresponding to them, there is an adaptation of the sensory attention. Such adaptations play a part in ataxic phenomena: (*e. g.* a patient unable to move a given finger at request except with open eyes, once having developed a motor adjustment by moving it so, can thereby keep on moving it for a little while after his eyes are closed), in the pleasure of rhythmic movements, the periodicity of the time sense, etc. The authors reject the innervation sense *in toto*, (in a later chapter they subject it to a thoroughgoing critique), likewise the muscle-sense, at least as a factor in judging weights. If such senses are the discriminating ones, they should tell us of the amount of force expended and prevent the illusion. The theory that weights are judged by the velocity with which they are lifted was suggested by Hering in a letter to Fechner, and was adumbrated by Lewinski and others, but has now for the first time been given a full experimental treatment. If weights are judged by velocity, the question of the perception of the movements and positions of the limbs becomes a cardinal one. On this point the authors accept the views of Goldscheider, locating the sensations chiefly in the joints, but reject his notion of an independent motion-sense.

Other sources of error were investigated, *e. g.* the *Zeitfehler*, or error introduced by the order of lifting the weights and the time interval between the lifting of the standard and of the weight to be compared, and the *Raumfehler*, or error introduced by the different distance and direction of the weights from the body of the lifter. Both of these find interesting and plausible explanation in the adaptation theory stated above. Special fatigue experiments showed, contrary to Fechner's "parallel law," a decline in the discriminative sensibility. A third weight lifted between the two to be compared, at first caused large errors, but the subject soon became able to neglect it. Weber's law follows as a necessary consequence of this theory, provided that light and heavy weights are judged by their velocities, for the just observable difference is then a difference of velocities, and it is a principle of mechanics that the change in mass needed to produce a fixed (just observable) change in the rate of a moving mass must vary proportionally to that mass. Practically however the law is much overlaid by confusing circumstances.

Voluntary Control of the Heart. EDW. A. PEASE. Reprint from the Boston Med. and Surg. Journal, May 30, 1889.

Mr. Pease reports a set of experiments upon a student in the Harvard Medical School who possesses this rare power. The subject was able with little irregularity of respiration to increase his pulse-rate for a few seconds in the proportion of about 17 beats per minute, and by holding his breath at the rate of 27 per minute. Other experiments show, however, that the power does not consist in any change in breathing, nor does it depend on increased blood pressure, voluntary motion, nor, according to the subject, on the fixation of any emotion or idea. The change of rate seems to be effected by a series of impulses which gradually weaken in force; the power to produce these is also easily exhausted. The subject, as seems general in such cases has a certain power over the ear muscles and others not commonly under control.

On the Observation of Sudden Phenomena. S. P. LANGLEY. Amer. Jour. of Sc. XXXVIII, 93, Aug. 1889.

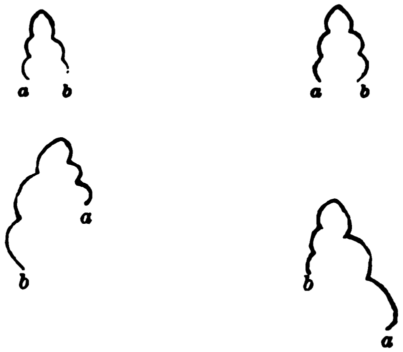
Reference was made in an earlier number of this JOURNAL (II, 24) to a device of Prof. Langley's for excluding personal equation in transit observations. He now presents a simple and ingenious instrument for practically excluding it in the observation of sudden phenomena (*e. g.* the emergence of stars from the dark limb of the moon, etc.). The detail of the instrument must be seen in the original; but in general it depends on the introduction of a double total-reflection prism, revolving in the axis of the instrument. The image of the emerging star, or whatever the phenomenon be, is thus made to appear in a different sector of the field according to the part of the second in which it occurs. On trial with a field divided into 20 sectors and an artificial star, one of the observers, without special practice, reduced his probable error for a single observation to about one fortieth of a second.

Ueber die galvanischen Erscheinungen in der Haut des Menschen bei Reizungen der Sinnesorgane und bei verschiedenen Formen der psychischen Thätigkeit. J. TARCHANOFF. Pflüger's Archiv, Bd. XLVI. pp. 46–55.

These experiments were made with the galvanometer of Meissner and Meyerstein, the deflections of the mirror being read off on a scale by means of a telescope. The instrument was so sensitive that the current in the N. ischiacus of the frog was sufficient to cause the scale to disappear from sight. The electrodes were applied to different parts of the body, principally to the outer surface of the hand and the inner surface at the base of the fingers. The currents of a state of rest were compensated for, and the subject was, of course, undisturbed and motionless. It was found that tickling was sufficient to cause a strong deflection of the needle. After a latent period of from one to three seconds, the current was at first weak and slow, and then so strong as to put the scale out of sight. The inside of the hand was negative, the outside positive. Electricity, heat and cold, the prick of a needle, caused the same effect, but not to such an extent. So did stimulation of the special sense-organs, the sound of an electric bell, the smell of vinegar and ammonia, the taste of sugar, etc. After the eyes had been closed for some time, simply opening them was sufficient to produce a current. Different effects were produced by different colors,—it is not stated what colors were the most irritating. But when no sensation was experienced, the mere imagination of a sensation was sufficient to produce a change of from 10 to 15 divisions on the scale. The idea of extreme heat was especially effective, and still more so if the hand, which was being tested was alone imagined to be in a state of perspiration. Any strong intellectual exertion produced a marked effect, even when the needle no longer responded to excitations of organs of sense; but a simple application of the multiplication table was not very effective. The effect of any contraction of a muscle was very marked. DuBois-Reymond first showed that a current is produced in the hand when the hand contracts, but he attributed it to the negative variation in the current in the muscle. It seems now that Herrmann is right in considering it to be a secretory current produced by the increased activity of the sweat-glands, for it is produced in every part of the body where there are glands, no matter where the muscular contraction takes place, and it cannot be detected in places where the sweat-glands are few or wanting. It is not so much the extent of the voluntary motion which regulates the amount of the current as it is the degree of conscious exertion that accompanies it; to fixate the point of the nose, makes a change of twenty divisions on the scale. If both points of attachment of the electrodes are well supplied with sweat-glands, the current is frequently found to move first in one direction and then in the other; if one only is so, the current is negative at that end. The existence of this current indicates an increased activity of the sweat-glands; it is well known that their activity develops the so-called secretory current. It is therefore proved that the course of nearly every kind of nervous activity,—from the simplest impressions and sensations, to voluntary motions and the highest forms of mental exertion,—is accompanied by an increased activity in the glands of the skin. The plethysmographic investigations of Mosso, François Frank and others show that the blood-vessels of the extremities take part in all kinds of nervous activity. But they contract, and hence they are not the cause of the increased activity of the sweat-glands. That must be due to a special excitation of the nerve-centers which regulate them. Its purpose is plain; all nervous activity means an increased accumulation of the products of decòmposition and a rise of temperature; the greater activity of the sweat-glands facilitates the elimination of the products of decomposition, and at the same time serves to bring down the temperature. C. L. F.

Untersuchungen über die Orientirung im Fühlraum der Hand und im Blickraum. J. Loeb. Pflüger's Archiv, XLVI, S. 1-46.

Loeb has made the important discovery that when we will to make equal excursions of the hand in the hand-space, the excursion actually executed is shorter than that willed, if the muscle concerned in making it is shorter than in a state of rest; and is longer, if the muscle is longer. The method of conducting the experiments was as follows. A person stands before a vertical table and makes with one hand, say the right, an excursion along a thread of definite length. At the same time he wills to move the left hand (which is not guided by a thread) over an exactly equal length: if this hand starts from a point higher up than the other and moves *upward*, it moves *less* than it should do; if it moves *downward*, it moves *more* than it should do. The more the starting-point of this hand is depressed (the hand which follows the string remaining at a medium height), the greater are its upward motions, the smaller are its downward motions; but the lower the starting-point, the longer are the muscles which raise the arm, the shorter are those which depress the arm. A simple experiment, by which anyone can convince himself of the sense of the illusion, is this. If, with the eyes shut, one attempts to draw a leaf with each hand, the two figures are very much alike, if the two hands are symmetrically situated. But if one hand is raised and the other lowered, the up-strokes of the one hand and the down-strokes of the other will be too short, or none of the four leaf-sides are drawn of the size intended to be drawn and supposed to be drawn.



The drawing goes each time from *a* to *b*.

The same thing holds for horizontal motions. But if horizontal motions are performed with the hand raised or lowered (but not horizontally displaced), the illusion does not occur; the muscles which effect *horizontal* motions are not put out of their resting-position by raising or lowering the hand. The error is very considerable in amount; the motion executed may be only one third as great or three times as great as that supposed to be executed. That it is the actual length of the muscle and not the tension it is under which causes the illusion, is proved by showing that the illusion is not produced by attaching weights to the hand; intended movements are executed very accurately even when the hand is restrained by a weight of nearly 6 kg.

Loeb finds, in opposition to Dr. Stanley Hall (*Mind*, No. XXXIII.) that gravity is of no effect in producing illusions of this kind; Dr. Hall happened not to try his experiments with the variable hand low down in its space, as well as high up, or he would have noticed that the illusion works against gravity as well as with gravity.

The muscles of the eye exhibit the same phenomenon; a distance looked over seems shorter in any direction, the further away it is in that direction. In indirect vision, the same thing holds, but with less exactness, and this leads Loeb to the conviction that the local signs of the retina are simply inhibited impulses of the will. There seems to us to be a lapse of logic in this, as well as in all other statements of the same theory. Granted that we estimate the distance of an object seen indirectly by a consciousness of the strength of the innervation which would be necessary to make us look at it, how do we judge of the required strength of innervation? In other words, if the imagined impulse of the will is the cause of the feeling of distance, that must itself have a cause in some preceding sensation, and nothing at all has been done to show that it is the impulse, and not *the sensation which determines the impulse*, that rises into consciousness, and effects our knowledge of the position of the object.

Loeb considers himself to have absolutely proved, by these experiments, that our knowledge of the extent and direction of our voluntary motions depends upon the impulse of the will to perform the motion, and not upon the sensations excited in the active organ of the motion. The two assumptions which the validity of this proof requires are, that no change in the sensations conveyed by skin, joint, tendon or muscle is produced by an unusual position of the organ (that is, that *sensation* is not a function of the present condition of the elements which communicate it, while the excitability of a muscle is a function of its state of excitation), and—what Loeb frequently affirms—that the constant hand and the variable hand are both moved by one and the same impulse of the will. To the consciousness of the present writer there seems to be, when the experiment is performed, the following processes going on in the mind. One is at once aware that, since the two hands are to start together and to end together, an accurate length is to be gone over by the variable hand by giving it the right velocity. During the tracing of the line (if it is done a little slowly) there is a constant estimation of the correctness of the velocity already executed (that is, a comparison of it with the velocity of the other hand) and a corresponding decision to increase the velocity or to diminish it for the rest of the excursion. When the hand is extended, the line is executed wrong; but how can one be sure that that is because a right degree of innervation has a diminished effect upon a muscle already contracted, and not because there is an error in the estimation of the amount of contraction which takes place in a contracted muscle? (It would be interesting to compare the steadiness of the velocity of the variable hand with that of the constant hand.) Nor does the assumption that both hands are moved by a single impulse of the will (and therefore by equal impulses) seem to us to be tenable. One is conscious, very plainly when the positions of the two hands are not symmetrical, of a rapid moving of the attention from one hand to the other, and that is, of course, the same thing as a rapid alteration of impulses of the will. Moreover, the two hands have totally different tasks to perform; the right has to follow a thread and to stop when it gets to its end; the left has to execute a perfect copy of the motion of the other. It is impossible that two such different commands as this should be executed by a single impulse of the will, nor is there any anterior reason for supposing that the two impulses required are necessarily equal in amount. The experimental results of Loeb's investigation are extremely interesting, but they do not seem to us to bear out his far-reaching conclusions.

C. L. F.

Ueber das mechanische Latenzstadium des Gesammtmuskels. DR. W. COWL. Separat-Abdruck aus den Verhandl. d. physiol. Gesells. Berlin. 1889.

In view of the difference of the results of Yeo's study of the "latent period" (see review, AMER. JOUR. PSY. II, 488) from those of Gad and more recent observers, the author has undertaken a repetition of Gad's experiments using the same methods and apparatus, and confirms his results, concluding that the "latent period" is due (though perhaps not solely) to a stretching of the muscle preliminary to its contraction. He prefaces the statement of his own work by a brief historical summary.

Ueber den Muskelsinn. GOLDSCHEIDER. Verhandl. d. physiol. Gesells. Berlin. Sitzung am 17 Mai, 1889.

The hypothesis of a sensation of motion, distinct from that of the positions beginning and ending the movement, is supported by the following considerations. 1. The sensation of motion becomes clearer as the rapidity of the movement increases, and attends movements of too short duration for the complicated processes of a judgment from the positions. 2. The just observable sensation of motion accompanies movements so very small that their limiting positions are probably indistinguishable. 3. Sensations of motion are clearly perceived before the direction of movement is clear. 4. The sensation of position can be temporarily removed by faradizing without destroying the sensation of motion.

In experiments on the lifting of weights it is well to use only single segments of the limb. The physiological conditions of the experiment are thus greatly simplified, with the result that in lifting the weight by a thread nothing of the sensation of encountering at some moment the resistance of an exterior heavy object is felt (a prominent sensation in lifting with more than one segment,) but only the more subjective sensation of heaviness (*Schwere-Empfindung*), of greater difficulty in executing the previously easy movement. This sensation of heaviness has its seat in the tendons; that of resistance, like that of motion, in the joints, and is called forth by the pressure of the joint surfaces upon one another. It suffers if there is motion in the joint at the same time. In lifting weights in the ordinary way both sensations are aroused.

As against the participation of an innervation sense in these judgments the following facts are adduced. 1. The sensation of weight is felt when the contraction of the muscle is produced by electrical stimulation or reflexly like the knee-jerk. 2. The limb may seem perfectly relaxed when it is still partly supported by muscular tension. 3. Movements may be made actively as well as passively which are too small to be perceived by the subject, and the limit of perceptible movements is raised by faradization in one case as in the other. 4. Innervation sensations do not mediate the sense of position, for that is almost entirely destroyed by faradization. 5. Certain illusions exist which should not be possible with an innervation sense. The consciousness of voluntary movement comes from the immediate succession of peripheral sensations of motion upon the genesis of the corresponding motor image. (The weight of evidence at present is very strongly against the existence of innervation sensations, and those whose theory of space perception involves them will have to bethink themselves of reconstruction. REV.)

Zur Frage der psychophysischen Messungen bei Geisteskranken. M. K. WALITZKAJA. Archiv f. klin. u. gerichtl. Psychiatrie v. Merschejewski. I, 17. Rev. by Kraepelin in the Allg. Zeitsch. f. Psychiatrie. Bd. XLVI, H. 2-3, S. 245*.

These experiments were made on 7 insane and, for comparison, on 5 sane subjects. In 4 of the 7 cases the diagnosis was general paralysis, in 2 progressive paralysis, in 1 paranoia. Simple reaction-times, choice-times, association-times, etc., and the times for adding digits were measured to a total number of 18,000. A table of the mean times required for these processes is given, also a table in which the association-times are further classified. But the very grave defects of method, (those which Kraepelin enumerates touch the control of the apparatus, the making of the protocol and the presentation of the results) rob the work of scientific value. The conclusions at which the author arrives are, in general, that in the beginning of paralysis there is an increase of automatic mental activity (including association) together with weakened volition; at a later stage the automatic activity also fails by degrees and simple perception is difficult. In nearly complete remission the automatic activities return fully, the voluntary and intellectual only partially; when the disease becomes acute the automatic processes again become prominent, the volitional difficult.

These results are not so very different from those found in the same field by Madam M. K. Walicka.

On some Facts of Binocular Vision. J. VENN. Mind. April, 1889.
"*On some Facts of Binocular Vision.*" J. H. HYSLOP. Mind. July, 1889.

Prof. Venn finds himself unable to get some of the experimental results brought forward by Dr. Hyslop in a previous paper, (reviewed, AMER. JOUR. PSY. II, 159.) In his reply the latter shows these differences to be more apparent than real and by no means important to the central idea of his paper, which was a criticism of Wundt's innervation theory. The discussion demonstrates as Dr. Hyslop notices, the great complexity of such experiments and the large part which personal variations in skill may have in the results.

L'agrandissement des astres à l'horizon. G. LECHALAS. Revue Philosophique, Juillet, 1888.
Une association inséparable: l'agrandissement des astres à l'horizon. M. BLONDEL. Ibid., Nov., 1888.

Continuation of the discussion, Ibid., Déc., 1888 and Fév., 1889.

Why does the moon look large at the horizon? The current explanation is that it then seems large because it seems far away, and seems far away because many things intervene. The object of Lechalas's paper is to review this theory in the light of certain experiments of Stroobant's (*Bul. de l'Acad. de Belg.* VIII (1884), 719; X (1885), 315). The most important of these were three. (1) He projected an after-image (of the sun in this case) upon a wall at such a distance that the after-image seemed of the same size as the sun, and found that distance to be always about 48 m. Plateau, who seems to have originated the experiment, found 51 m. with the moon. This shows, Stroobant contends, that these bodies always appear at the same apparent distance, and is therefore a definite disproof of the distance theory. Lechalas objects that the moon really seems much further than 50 m., and Blondel shows in his paper that while the experiment shows the constancy of the retinal image, it does not show the constancy of the apparent distance, appearances being for the mind and not the eye. (2) The second experiment, the force of which Lechalas admits without abatement and to which he assigns a physiological explanation, was as follows: The experimenter produced near the ceiling of a perfectly dark room two electric sparks 20 cm. distant from each other, and in a horizontal direction at an equal distance from the eyes two other sparks whose separation could be varied. He found that when both seemed equal this separation was only about four-fifths that of the pair on the ceiling, and that the illusion persisted when the observation was made lying down. This, however, would account for only about half the difference of the apparent size of the moon at the horizon and zenith, and Stroobant found that when the rising moon was observed at the zenith by a mirror set at 45° it yet appeared larger than when actually there, a difference which he attributed to the faintness of the light at the horizon, verging thus toward Berkeley's view. (3). His third experiment was upon this point; the light of a lantern thrown into the eyes of the observer reduced the apparant size of the moon, and certain laboratory experiments seemed confirmatory; with the latter, however, Lechalas is dissatisfied. On the whole he believes the current explanation valid in part, but needing the addition found in Stroobant's second experiment.

Blondel's explanation goes deeper and makes the whole thing, including Stroobant's second experiment, psychic. The eyes see the moon always of about the same size; the illusion enters when the imagination introduces the element of distance. And this is no such simple thing as the common theory makes it; it may be present when there are no intervening points, as when the moon rises over a wall. One element in it is the shape we give to the sky (a low arch) by reason of the great preponderance of experience in horizontal extension as compared with that in vertical. With this co-operates the seeming accessibility of the moon when red and low and apparently in our atmosphere, "for then the tactile image supplants the visual image." Granted the apparent difference of distance, the false perception results from a piece of sensory logic: Of two objects which have the same visual angle, the further is the larger; the moon is further at the horizon than at the zenith; therefore, she is larger at the horizon. It is this aspect of the phenomenon that interests Blondel chiefly, and he goes on to show that the elements of these perceptions-by-inference are dependent on circumstances, (a red moon at the horizon seems large, but one reddened in eclipse seems small); "alone, each detail explains nothing; there must be grouping and generalization, in order that a perception result from it, as the conclusion results from premises of which one at least is universal;" there is an unconscious syllogism. A syllogism is then nothing artificial, but the natural process of unconscious as well as conscious thought. This illusion is one of the best examples perhaps of a universal and inseparable association, one which is no longer a process but has become a single act.

Ueber die scheinbare Grösse Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder. GÖTZ MARTIUS. Philos. Studien, Bd. V, H. 4, S. 601.

In general, objects having large visual angles appear large and those having small visual angles appear small. But this general principle is materially limited in its application in the case of objects at different distances by the experience of the subject; the apparent size of objects does not decrease nearly as rapidly as their visual angles do. The experiments of Dr. Martius were to determine the accuracy of this sensory judgment or perception by inference. He hung up vertically before a screen standard rods of 20, 50 and 100 cm. long and before a similar screen at a greater distance, other similar rods in succession varying slightly in length from the standard. The subject was required to choose that which seemed nearest the standard. His results are summed up in the following table. The standard rod was at a constant distance of 50 cm. from the eye of the subject; the comparison rod was in one series at 2.50 m. (left hand table), and in the other at 5.25 m. (right hand table) behind the standard; the figures are the excess in cm. of the rods which seemed eqnal to the standard rods over the length of the latter.

	Distance 50 cm. + 2.50 m.			Distance 50 cm. + 5.25 m.		
Length of standard in cm.	20	50	100	20	50	100
Observer M.M.	0.62	3.87	7.75	1.67	7.62	6.62
" G.M.	1.62	6.62	9.25	1.92	9.00	10.00

The table indicates according to the author that "the comparison-magnitude which seems at different distances equal to a given [standard]-magnitude increases constantly with the distance, but very slowly;" that "the absolute difference of the comparison-magnitude, which seems at a given distance equal to the standard-magnitude, increases with the latter;" and that "it is probable that the relative difference remains nearly constant [for all] at the same distance." For the further discussion of these results, which tell, as far as they go, in favor of the empirical theory, the reader is referred to the original.

III.—HYPNOTISM.

Der Hypnotismus, seine Bedeutung und seine Handhabung. A. FOREL. Stuttgart, F. Enke, 1889. pp. 88.

A part of this pamphlet originally appeared in an article in the *Zeitschrift f. d. ges. Strafrechtswissenschaft* (reviewed, AMER. JOUR. PSY. II. 316), and is now published with additions to meet the desire for a brief account of the more important facts of hypnotism. The scientific standing of the writer and his experimental knowledge of his subject make his work one to be most highly recommended. Among the additions is a section on the subjective aspect of hypnotism, in which are reprinted from the *Münchener Med. Wochenscrift*, Dr. Bleuler's experience as a subject, (noted below) and the author's own experience in auto-hypnotization (reviewed, AMER. JOUR. PSY. II., 509).

Zur Psychologie der Hypnose. DR. E. BLEULER, of Rheinau. Münchener Med. Wochenschrift, No. 5, 1889.

The self-observation of an intelligent subject always has value and even more by reason of its rarity in hypotism. Dr. Bleuler entered the experiment with full will to be hypnotized, but endeavored to withhold himself from suggestion to learn its power. He thus describes his sensations on the first establishment of hypnosis. "My condition was now that of a pleasant and grateful repose; it came over me that I had no need at all to change my position, which under other circumstances would not have been continuously quite comfortable. Mentally I was completely clear, observing myself; my hypnotizer could confirm everything objective that I afterwards told. By the suggestions that followed, the content of my conscious thought was not otherwise influenced than in waking; nevertheless they were in great part realized." On being told that he could not straighten his flexed arm, he felt his biceps tighten, entirely against his will, and counteract the extensors. On other occasions this was not felt; his will seemed then to have lost control over his muscles, or even to be itself paralyzed. He was able to reflect critically that the suggestion of an anæsthesia was made too early in the experiment, but felt pricks only as touches with a blunt edge. After repeated suggestion that he should wake next morning at a fixed hour, he passed a restless night waiting for the time to come; when the suggestion was otherwise given his sleep was undisturbed. Though he could long contend against a suggested action he was generally obliged to yield. Each new one, even if it was the cessation of action, seemed for the instant unpleasant. Suggestions of a complicated action could easily be resisted as wholes, but not when the successive acts necessary to its accomplishments were given separately. A post-hypnotic suggestion was with difficulty kept from fulfilment till wiped out by a night's sleep. Once an hallucination of taste was produced. When amnesia was suggested, but not very vigorously, there was difficulty in recollecting what happened, (once a brief period was wholly lost), and the subject could not fix the order of events. He was roused against his will by suggestion in about 10 seconds and without unpleasant symptoms.

Suggestive Therapeutics. A treatise on the nature and uses of hypnotism. H. BERNHEIM. Translated by Christian A. Herter. New York, G. P. Putnam's Sons, 1889, 8vo., pp. 420.

Of the French original of this work we said in May, 1888, "we regard [it] as on the whole the most scientific of the many works that have appeared in France within the present decade upon this subject, and we deem it a matter of serious regret that writers representing this method and stand-point were not chosen by the publishers of the International Scientific Series to present the subject to English and American readers, in preference to such thorough-going partisans of the school of Charcot, which . . . . . has been latterly so reluctant to accept the far better methods and results of Nancy." The regret then expressed has been met by the translation before us. Dr. Herter has had opportunity for observing the phenomena of which his author writes, with Prof. Forel of Zürich, himself a prominent representative of the Nancy school, and also in Paris at the Salpêtrière. His translation is readable, and the American public is to be congratulated on having the better side of the Paris-Nancy debate so well represented.

Hypnotism: its history and present development. F. BJÖRNSTRÖM. Translation from the second Swedish edition by Nils Posse. The Humboldt Library No. 113. pp. 124, 8vo. New York, 1889.

As long as comparatively few of the many foreign works on hypnotism are rendered into English and as still fewer are written in English, it becomes a matter of importance which books are chosen for translation, when as in this case the work is distinctly addressed to a large public and is published in a very accessible form. The work of Dr. Björnström is a clear and orderly exposition of the chief points in modern hypnotism and is in many ways the most recommendable book for initiation into the field we yet have in English. Its history, the methods of inducing it, its stages, its physical and psychical effects, the importance of the term "suggestion," the applications of hypnotism in disease, education and law, its abuses and dangers are all included in small compass, precluding any detailed exposition, but fully enough for the general reader. The weak point of the volume is its lack of distinction between facts and the interpretations of them. The question of unilateral hypnotism and especially that of "mental suggestion" occupy too much space, and the conclusion in favor of these remote phenomena is accepted much too readily. This as well as the account of Parisian pseudo-hypnotism give the volume an unequal and even semi-sensational turn.

J. J.

Der Hypnotismus. A. Dergauc. Laibacher Zeitung. Mai, 1889.

A series of popular articles giving clearly though not always critically a survey of modern facts and methods in Hypnotism together with some account of its therapeutic, educational and forensic aspects.

Der Hypnotismus, seine Stellung zum Aberglauben und zur Wissenschaft. Eugen Dreher. Berlin, 1889. pp. 38.

After a needlessly prolix discussion upon the distinctions between science and superstition, Dr. Dreher proceeds to give an account of the chief facts of hypnotism, and repeats the well-known story of the rescue of the topic from fields of charlatanry. One of the most positive points in the work is the denouncement of all telepathic hypotheses as falling into the realm of the superstitious, and as entirely opposed to scientific notions. The pamphlet makes very interesting reading, but is not particularly important.

A Clinical and Forensic Study of Trance. Edw. P. Thwing. Medico-Legal Journal, June, 1889.

That the trance state may give rise to complicated legal questions, goes without saying. In this article the state itself and a number of these questions are taken up in a sketchy way by Prof. Thwing, with a view to directing attention to them. The great question to which these are mostly corollaries, is the immensely intricate one of "the relation of automatism to responsibility."

Hypnotism. G. T. W. Patrick, Ph. D. North-western Journal of Homœopathy, May, 1889.

Prof. Patrick goes over the chief phenomena of hypnotism in an eminently sane and scientific spirit. Current information, even as it reaches the general medical reader, is apt to be more or less unreliable, and it is a matter of congratulation when those who are neither unbalanced enthusiasts nor bigoted conservatives engage in the work of popularizing it.

Leçons sur le grand et le petit hypnotisme. Grasset. Reported by G. Rauzier, Revue de l'hypnotisme, Mai et Juin, 1889.

These three lectures set forth the characteristics of the Paris and Nancy schools of hypnotism, and Grasset's harmonizing of their opposing views. The chief argument of the Paris school (that of *le grand hypnotisme*) against the school of Nancy (that of *le petit hypnotisme*), is that they have no sure way of detecting simulation. The chief argument of the latter against the former is that all their characteristic phenomena are the result of suggestion. Grasset denies both these arguments; there are guarantees against simulation besides those used in Paris, and there are physiological effects besides those produced by suggestion. There are, indeed two forms of hypnotism, or rather the one neurosis (for Grasset regards the whole thing as morbid) has two groups of symptoms; one (*le grand hypnotisme*) is found only in connection with hysteria, and not always even there. The just claim of the Paris school to its title must rest on its having secured scientific attention for the outlawed phenomenon. In point of number of cases, of having caught the central point of the thing (suggestibility), and of therapeutic application, the Nancy school deserves the name of "the great hypnotism."

The Study of Hypnotism in France. Joseph Jastrow. Christian Union, Sept. 26, 1889.

The author outlines with characteristic clearness and interest the history and present status of knowledge in regard to hypnotism, distinguishes the views of the investigators of Paris and Nancy, describes the post-hypnotic phenomena and positive and negative hallucination, and points out the possible usefulness of hypnotism as a remedial agent together with the questions of criminal responsibility to which it may give rise.

Solution du problem de la suggestion hypnotique. Amédée H. Simonin. E. Dentu, Paris, 1889. pp. 129.

This little book sets forth what the author believes to be the solution of the problem of hypnotic suggestion. It also sets forth most forcibly his total ignorance of the way to a real solution of the problem and of the fundamental physiological conceptions necessary for it, both of which would at once appear, were it worth while to make citations.

Ueber psychische Beobachtungen bei Naturvölkern. Adolf Bastian.

Die Magiker Indiens. Friederich von Hellwald.

Both from the Shriften der Gesellschaft für Experimentalpsychologie zu Berlin. III Stück. Leipzig, 1890. pp. 32.

The reward for pushing one's way through the many twistings and turnings thickly strewn with obstructing parentheses and scraps of polyglot illustration, that characterize the writings of this suggestive but obscure anthropologist, is in the present case a very interesting though arbitrarily eclectic survey of that field of mental action, that is common ground to science and superstition. On the one hand we find the same pseudo-scientific pretenses that succeed with the unlearned even amongst the elite, in the customs and thought-habits of savage people; on the other hand we find the scientific portions of this field, the various methods of producing hypnotism, etc., capable of explaining the more wonderful portions of savage doings and conceptions. Many pertinent illustrations of these facts are given and they lead the author to conclude that it is precisely in primitive peoples that we find as more or less normal most of those phenomena producible in us only artificially and not without danger to the subject; so that the simple observation of well prepared travellers would lead to an unsuspected extension of the field of research."

Quite in the same strain von Hellwald cites cases in which magic, self-induced hypnotism and other factors enter into the complex operations of the fakirs and other oriental priest classes. J. J.

Des hallucinations négatives suggérées. BERNHEIM. Revue de l'hypnotisme, Fév., 1889. Réponse à M. le Professeur Delbœuf.

In commenting on a paper of Bernheim's in the January number of the *Revue*, Delbœuf insisted on the very great and intelligent part played by the subject in case of negative hallucination, in language which might be construed to mean that the whole thing was simulation (*vide* AMER. JOUR. PSY. II, 324). To prevent such a misapprehension, Bernheim again defines his position, asserting the complete freedom of genuine cases from all simulation. Unilateral blindness in hysteria is equally real and equally psychic, and occurs where the ignorance of the subject, both of the defect and its connection with her disease, guarantees the impossibility of simulation. With the hypnotic subject the sensations reach the cortical centers, but fail of the further processes, whatever they may be, needed to bring them to consciousness; they are, so to speak, unconsciously perceived. With hysterics these further processes are not lacking, but "the imagination of the subject without her knowledge neutralizes" the sensation.

Quelques remarques sur suggestion. AUGUST FOREL. Revue de l'hypnotisme, Avril, 1889.

In this somewhat rambling paper, Prof. Forel touches upon a number of interesting points in regard to suggestion and auto-suggestion (which are the same as far as the subject is concerned), the process by which the operator secures control of the mental machinery of the subject, the unpleasant after-effects of hypnotization (due to auto-suggestion and to be suggested away by the operator), means of making subjects auto-hypnotizable, etc., etc. Apropos of the discussion of Bernheim and Delbœuf, he mentions the arrest and recall of the menses as unsimulable by the subject. In his opinion, "it is not only the will of the subject which is sometimes more, sometimes less completely directed by the hypnotizer, but his whole cerebral dynamism, sensorial (centripetal), motor (centrifugal), and intellectual (central) alike."

Les perceptions inconscientes de l'hypnotisme. A. BINET. Revue Scientifique, Fév. 23, 1889.

Binet complains of the misconceptions arising from confusion of terms in hypnology, citing as an example the recent papers of Liégeois, Bernheim and Delbœuf on negative hallucination, which were supposed to refute the views of his party, whereas they were actually in agreement. He now defines that phenomenon, and enumerates the facts agreed upon. As a name for the phenomenon, Binet himself with Féré, proposed "systematic anæsthesia." To avoid confusion, however, he is willing to accept "unconscious perception" or "negative perception," terms certainly more fitting, in view of recent experiments, than "negative hallucination."

De l'amnésie rétroactive dans le sommeil provoqué. BERNHEIM. Revue de l'hypnotisme, Juillet, 1889.

Certain subjects on waking from the hypnotic trance, are without memory, not only of what has happened during that state, but of events immediately preceding it, and this may happen spontaneously with persons that have never before been hypnotized nor seen others hypnotized. A case of this kind is described by Bernheim. No explanation is offered, but the similarity to retroactive amnesia in fevers and alcoholic delirium is recalled.

Note sur l'enregistrement des excitations portées sur une région anesthésique du corps chez les hystériques. ALFRED BINET. Compt. rend. de la Soc. de Biologie, séance du 12 Jan., 1889.

The motor effects now reported are in the line of the curious optical effects secured by the same experimenter by the stimulation of anæsthesic areas on the bodies of hysterical patients (see abstract, AMER. JOUR. PSY. II., 324), and are equally suggestive with reference to the conscious unconsciousness of some hypnotic subjects. He finds that stimulation of an anæsthesic area is followed by a slight, unconscious muscular excitation, the effect of which is to be seen in myographic tracings taken on the muscle masses of the limbs, trunk and face. Stimulation of a sensitive area produces a less widely irradiated excitation in many subjects than stimulation of an anæsthesic area, and in other particulars also there are variations of result. The rhythm of stimulations is sometimes followed, and the duration and intensity of the stimulus is of effect on the trace, which make it seem probable that, in spite of the subject's unconsciousness, psycho-motor centers take part in the response.

Recherches sur les altérations de la conscience chez les hystériques. A. BINET. Revue Philosophique, Fév., 1889.

It is well known that the anæsthesias of hysteria are vastly different from those of organic lesions, and the researches of M. Binet make this yet more apparent. The tests were made out of sight of the patient, without previous suggestion or hypnotization, and his results are, he thinks, symptomatic of the disease. It appears that, while simple pricks and touches produce no reaction, stimuli with some meaning (a dynamometer placed in the hand, or a pencil in the position for writing), may provoke a characteristic response of adapted motion. In some subjects, the presence of all the kinæsthetic senses and that of pain can be demonstrated. Furthermore, this action is not independent of consciousness. The thoughts of the subject find expression (in the case of automatic writers) on the anæsthesic side, and the sensations of that side reach consciousness though not with their normal character, tactual sensations generally calling up visual ideas. Binet is inclined to suspend judgement on the question of the conscious perception of these sensations by a second "personality," but argues from his experiments that this doubling of "personality" cannot arise in dissociation or splitting of the ideas into two independent groups, as some have contended. The elaboration of the responses and their connection with consciousness support the cortical theory of hysteria long taught by Charcot. Regretable as it is that such important experiments must be conducted upon such uncertain subjects as hysterical patients, and that M. Binet is not over-skeptical in regard to the action of the magnet, his experiments do not fail of a very great interest.

Ueber Hypnotismus und Suggestion, sowie deren therapeutische Anwendung in der ärztlischen Praxis. FRANZ MÜLLER. Wien, 1889. Moritz Perles. pp. 20.

Believing that there is still a large number of physicians who are skeptical regarding the applications of hypnotism in medicine, Dr. Müller prepares for them an excellent common-sense statement of what is meant by suggestion and how it acts. He takes forcibly and throughout the position of the Nancy school and insists that the process is psychical in every phase. After citing the usual cases of the influence of mind over body, he reminds us that hypnotism is not a panacea, as some claim; it has its distinct limits, largely the same as the limits within which such factors as a cheerful mien, impressive and hopeful manner are effective. "The domain of the therapeutics of suggestion is occupied primarily by the hysterical complex of symptoms; secondly, paralyses, cramps, and neuralgias caused by psychic shock; thirdly, neurasthenic troubles and sleeplessness, and finally pains and neuralgias." A point noted in combatting the purely physical theory of hypnotization may be cited. Dr. Müller asks why, if as many claim a bright light produces hypnosis, do not ophthalmologists find amongst their numerous patients cases of spontaneous hypnotism when the ray of light from the ophthalmoscope is thrown upon the retina? The query is certainly in point, and argues that the light is efficient when its effect is expected. J. J.

Ueber den Hypnotismus und seine Verwerthung in der Praxis. Dr. W. BRUGELMAN. Berlin, 1889. pp. 20.

This pamphlet is interesting as an additional endorsement from German soil of the therapeutic application of hypnotism. The position taken by Dr. Brugelman presents nothing peculiar. He regards hypnosis as closely affiliated with normal sleep, and as an important instrument in the hands of a competent physician for alleviating not all but an essential portion of human ills. The pamphlet is deficient in not recognizing the characteristic distinctions between the schools of Nancy and Paris, but in his own practical advice and treatment of cases he follows the former. In discussing the causes of the variations in the percentages of hypnotisable persons Dr. Brugelman makes the apt suggestion that the local environment is a most potent factor; in Nancy, for example, there has grown up a generation accustomed to being hypnotised and to regarding it as an every-day process. Hence the percentage is high.

Die Suggestionstherapie und ihre Technik. EDWARD BAIERLACHER. Stuttgart, 1889. pp. 57.

This is a purely practical treatise, dealing with the chief facts, methods and uses of the hypnotic process. The essay falls into two parts, the first on the process and the second giving a list of cases with appropriate criticisms. The former gives a convenient résumé of the chief points necessary for the practitioner, the latter shows the large variety of diseases to which the treatment is applicable. The success attending the treatment elsewhere will doubtless soon lead to a speedy extension of it throughout Germany.

Clinique de Psycho-Thérapie suggestive fondée à Amsterdam par les docteurs A. W. VAN RENTERGHEM ET F. VAN EEDEN. Bruxelles, 1889. pp. 92.

This comprehensive report read at the recent Congress of Hypnotism in Paris, places in convenient form the results of the "suggestive" treatment of 414 cases covering a period of two years. These two physicians took as their model the clinic of Liébeault in Nancy, and proceed entirely on the methods of the Nancy school. They have a suite of five rooms in a hotel in Amsterdam, comfortably arranged with easy chairs for the patients, with precautions against ill effects, and provision for pleasant impressions upon awakening. Their methods are strictly scientific, and they are actuated by a desire of demonstrating the power of suggestive-therapeutics when scientifically applied. They confine their efforts simply to the cure of the patient, suggesting repeatedly that the pains will vanish, that functional irregularities will disappear, that the power over a lost limb or a lost sense will return, that sleeplessness or mobid appetites will no longer trouble the patient, and so on. Each case must be treated on its own merits and according to the character of the individual. The suggestion must, where possible, be aided by initiating in the hypnotic state the effects desired after awakening. Regarding the results they tabulate all their cases, appending histories in typical instances, and subdividing them into the following ten divisions, of which the first five are affections of the nervous system, and the others of parts other than nervous. The number of cases treated under each division is added. 1. Organic (?) affections, 29. 2. Serious neuroses, hysteria, 40. 3. Mental diseases, 60. 4. Neuropathic troubles, 164. 5. Various nervous pains, 68. 6. Internal functional troubles, 27. 7. External functional troubles, 17. 8. Fevers, 1. 9. Menstrual troubles, 7. 10. Anæsthesia for surgical purposes, 1. Taking first the statistics in general we find 53 per cent. men and 47 per cent. women; we find about 4 per cent. remaining uninfluenced by the suggestions, 56 per cent. falling into a light sleep, 32 per cent. into a deep sleep, and 11 per cent. reaching the stage of somnambulism. Of the 414 patients 9 were between 1 and 10 years of age, 46 between 11 and 20, 203 between 21 and 40, 131 between 41 and 60, and 25 between 61 and 80. On 20 per cent. the treatment had no beneficial effect; on 26 per cent. it produced a slight or passing amelioration; on 27 per cent. it produced a marked or permanent amelioration; and 28 per cent. were cured. Regarding next the nature of the disease the vast predominance of nervous troubles at once suggests the special field of hypnotic therapeutics; of non-nervous disease classes 6 and 8 have the greatest chances of cure by this treatment. Of 29 organic affections we find but a single case of cure, (and even that in doubt), showing the absurdity of the panacean claims sometimes advanced. The more serious neuroses are almost all decidedly improved, but only 9 in 40 completely cured; mental diseases show a small percentage of cure and a large percentage of negative or slight results. Class 4 shows an excellent record, 39 slightly, 37 markedly improved and 47 cured, while class 5 shows 11, 19 and 17 in the same divisions. The tables contain many more interesting facts, and will in the future be constantly referred to as amongst the first sufficient data for a reliable conclusion regarding suggestive therapeutics. This conclusion must declare the method in comparison with other methods an excellent success, particularly with functional troubles of the nervous system, slight and serious. J. J.

Das Doppel Ich. MAX DESSOIR. Berlin, 1889. 8vo. pp. 42.

In this very entertaining lecture of a prominent member of the Berlin Society for Experimental Psychology, we have a consistent elaboration of the theory that has played quite a rôle in the history of Psychology, that the mind is dual in nature, that there are two of each of us. But this *alter ego* is not the tenant of the other half of the brain as Wigan had it; it is rather a secondary form of consciousness, an under consciousness which in ordinary cases occupies the humble quarters assigned to it but occasionally makes itself prominent and exhibits curious and bizarre forms. When we are deeply engrossed in work and yet something within us counts the strokes of the clock unknown to the worker; when we practice ourselves in this art so that we can keep up a lively conversation and at the same time perform intricate additions and multiplications, then both our egos are working at once. Ordinarily the upper consciousness alone does the work of direction, but occasionally the under consciousness takes the reins. When we automatically do things that are habitual and take no cognisance of it, as when we abstractedly take the night-key with us on going out and then suddenly start to go back for it, unaware that the under-consciousness has already attended to it, we are in the hands of the "*Doppel Ich.*" So much for the normal. This germ of a second personality which we each carry with us may develop unusual forms until it passes step by step into the pathological. When in dreams we take up the thread one night where we left it the night before, when the drunkard remembers in one intoxication what he did in the previous one, (though in the sober condition unable to do so), these changes of bodily condition seem the sufficient starting points for a disintegration of the self. From this the steps are gradual to the seizures of the epileptic, to those cases of sudden amnesia in which every faculty seems to be lost and then as suddenly regained, and finally to such complete instances of dual personality, as Dr. Azam's case of Felida X, whose normal conditions form a complete chain by themselves, but the links of which are alternately interrupted by the secondary states equally continuous amongst themselves, but no remembrance of the normal condition remaining in the secondary, nor vice versa.

But the subject has an experimental side as well. Hysteria and hypnotism are the fertile fields in which such experiments flourish. In one hypnotic trance patients remember what they did in former ones though they forget it in the intervals; in the execution of post-hypnotic suggestions we see all shades of relation between the hypnotic and the waking *egos*, some patients falling into a sort of hypnosis again, others performing the act unconsciously, and still others doing it consciously and offering a lame excuse for it. Still further, a few cases have been described, notably one by M. Pierre Janet, in which the hypnotic personality regularly assumes a constant personality but one differing from the normal and entering into the most complicated relations with it. Indeed a third personality emerges by the hypnotization of the abnormal personality. The proposition which Dr. Dessoir reaches and in which M. Janet and Mr. Myers concur is that the hypnotic state consists in "an artificially induced predominance of the secondary *ego*." To prove this a large number of the experiments, some very ingenious and others very inconclusive, are undertaken to appeal indirectly to the ordinary consciousness, which in the hypnotic state is the subordinate one, and gain the evidence of the two personalities existing side by side but with the usual relations reversed.

The point of view thus taken is certainly an interesting one, but is it not expressing, with an undue emphasis upon that unknown factor of personality, the current doctrine that in hypnotism we have an automatic state, a loss of voluntary control and an exaggerated suggestibility in all directions? The "double-*ego*" is a convenient phrase for bringing into connection various groups of facts, but in its extreme form it loses its utility, and as a theory of hypnotism it is neither so novel nor so important as its upholders believe. J. J.

Mary Reynolds: A Case of Double Consciousness. S. WEIR MITCHELL, M. D. Reprint from the Transactions of the College of Physicians of Philadelphia, April 4, 1888.

Mary Reynolds was born in 1793 and died in 1853; her case is therefore not of recent observation, and Dr. Mitchell is compelled to depend upon testimony. The contribution is nevertheless a very acceptable one. At about eighteen the girl began to have hysterical "fits." A little later, in a period of prolonged sleep, she experienced her first change of consciousness, and thereafter for fifteen or sixteen years continued to pass at irregular intervals from one state to the other, being left at the age of 36 in the second stage. In the first she was retiring and melancholy; in the second, fond of society and light-minded. When however she finally rested in the latter state the mental disturbances sometimes attending it gradually disappeared; she became by degrees more sober also, but without losing the prevailing color of the state. Specimens of her script in the two states by their remarkable similarity point a moral upon the treacherousness of popular testimony as to changes of handwriting in such cases. The numerous particulars however which multiply the interest of the original do not lend themselves to summary here.

Des hallucinations suggérées a l'état de veille. E. YUNG. Revue de l'hypnotisme, Mars et Avril, 1889.

Prof. Yung has made a large number of experiments in the suggestion of mild and transient hallucinations (*i. e.* hypnotic suggestions in embryo) to normal people—not in special and unusual circumstances, but in those of every-day life. The experiments succeeded better, but by no means exclusively, with women and children and the uneducated. They require a certain state of mind in the subject, which it is the business of the operator to produce by authoritative manner, earnest assertion, and in general by the arts of the "magnetizer." The author has some 30 drawings made by students in his microscopical laboratory of objects which they saw and drew from blank slides after authoritative description by him of what they were to find there. By means of the parlor games of the "magnetized card" and the "scented coin," both fully described in the paper, Prof. Yung secured a large number of observations. Of 600 experiments made with the first, only 83 (13.8 per cent.) failed completely: 68 on men, 15 on women. The remaining 517 fall into 4 groups: 212 recognized the "magnetized card," according to instructions, by a shock in the muscles of the fingers, the hand, the arm, etc.; 95 to whom only a general touch sensation was suggested had various tactile sensations; 52 saw the card move on assurance that it was so to be recognized; and 158 knew it by an odor. Those that have been hypnotized are, as might be supposed, most susceptible to these hallucinations, but Yung believes that everyone sufficiently prepared, as described above, may become a subject.

IV.—MISCELLANEOUS.

Elemente der Psychophysik. GUSTAV THEODOR FECHNER. Zweite unveränderte Auflage. Leipzig, 1889. Breitkopf und Härtel. 2 vols., pp. 336 and 572. 16 mks.

This second edition of Fechner's epoch-making work follows closely upon the death of its author. The work has long been out of print, and copies advertised for sale were always eagerly sought. Fechner himself did not care to undertake the necessary revision for a second edition, nor did he think it right to re-issue the first just as it stood. Prof. Wundt is the editor of the present edition, which differs in no way from the first, except in a few verbal changes and the incorporation of notes referring to Fechner's later works. There is also appended a very complete bibliography of Fechner's work, mainly by Dr. Rudolph Müller. When we remember that in this work the problems of psychophysics find their first systematic statement and elaboration, that many of the observations therein described still remain as the basis of current views, and that the historical value of the book must continually increase, we feel the necessity and propriety of the re-issue of this treatise, and offer our thanks to both editor and publisher for again placing the most important outcome of a talented savant's life in the reach of all students of psychophysics. J. J.

Handbook of Psychology: Senses and Intellect. JAMES MARK BALDWIN, Ph. D. New York, Henry Holt & Co., 1889. pp. 325, 8vo.

In the present aspect of Psychology a restatement of its problems and results in a convenient and readable form would be welcomed by a large company of students, who on one hand are repulsed from the host of individual systems by their unprovable statements, and on the other are equally deterred by the scattered results of the new psychology. To these the present volume will be a disappointment. It is difficult indeed to find its *raison d'être;* there are abundant equally meritorious text-books without adding to their number. As representing the distinctive field of modern psychology, which in part it aims to do, it is hopelessly deficient in the statement and arrangement of fact, as in the presentation of general views and theories. J. J.

Know Thyself; or Psychology for the People. A. W. HOLMES-FORBES. Dublin, 1889, pp. 52.

The aim of this pamphlet is to present some of the more general laws of mind in readily comprehensible form. The author is a dualist and finds in the mind by introspection three parallel orders of phenomena, one arising from the immaterial soul alone, one from the body alone, and one from their union. It is hardly necessary to say that his psychology is neither physiological nor experimental.

Sinneswahrnehmung und Sinnestäuschung. Dr. E. REHFISCH. Berlin, 1889. pp. 62.

The author's intention is to present for the benefit of the "laity" a view of the processes of sensation and sense-illusion in the light of recent physiological progress; and in this he succeeds most admirably. The point of view is typically that of the new Psychology, and the entire presentation most satisfactory. The first half deals with the physiological substrata of sensation, accentuating the different factors of the process in relation to the various centres of the brain; in so doing Dr. Rehfisch does not confine himself to a matter-of-fact series of statements, but surrounds the exposition with many interesting and pointed illustrations. The second portion of the treatise takes up the morbid side of sensation, making the usual distinctions and enforcing them with cases in point. There is nothing original about the work, but it is simply what it pretends to be,—an easily comprehensible general review of the problems of sensation.
J. J.

NOTES.

An interesting illusion affecting the sense of resistance is noticed by Goldscheider in the *Centralblatt für Physiologie*, No. 5, 1889. When a weight is held suspended by a cord and the hand is carefully lowered till the weight rests upon some support and relieves the hand of its downward tension, there is a sensation of resistance to further downward motion; and the resistance is located at the place of the weight. When the weight is sufficiently heavy, the illusion may equal the sensation of touching the support with a rod. The explanation is evidently in the persistence of the muscular tension originally required to support the weight. There is no upward pressure on the skin nor stimulation of any other superficial organs; the illusion and the perception of resistance rest upon some deep-seated sensation, probably that in the joints. The same sensation is present of course in touching with a rod or with the finger alone, though the dermal sensations are added; it is therefore in part at least in the joints that this important factor of belief in the reality of the external world is located. The projection of the sensations of resistance to the end of the rod or to the weight depends on the presence of other ideas which give a preconception of that location.

Thomas W. Engelmann, the discoverer of the photometric bacterium, has continued his observations upon another group of these light-responsive organisms, (*Botan. Zeitung*, No. 42–45, 1888, cf. also *Pflüger's Archiv.*, XLII, p. 183). These are mostly "sulphur bacteria" and all contain a diffuse red pigment (bacteriopurpurin), from which he calls the group *Purpurbacterien*. Their behavior toward light is something as follows: When the illumination is not too long continued, their activity increases with the intensity of the light. Long illumination, as complete darkness, brings them to rest. Sudden decrease of intensity causes a sudden backward movement (*Schreckbewegung*) of some time ten or twenty times their length, followed, if the light continues at a lower intensity, by slower forward movements. Sudden increase of intensity causes more rapid forward movements if the rate is not already very great. In a micro-spectrum they gather thickest in the ultra-red, next in the orange-yellow, and less toward the violet, their preference corresponding with the place of greatest absorption by the bacteriopurpurin. The effect of light on their movements Engelmann believes to be directly proportional to this absorption. Like chlorophyll, the pigment sets free oxygen in the light, a fact neatly demonstrated by the thronging of bacteria sensitive to oxygen about the *Purpurbacterien*.

M. J. Fontan, like Bernheim and others, has made the observation that in the cases of negative hallucinations in hypnotism, the physiological processes are carried out though the psychic ones are inhibited. When a subject acts out the suggestion that he sees nothing red, his pupil none the less contracts when a bright red is shown and dilates when exposed to a dark red. Better still; let the suggestion be given that red is invisible but when a bell is sounded the vision for red will return. Now let the subject gaze fixedly at a red light and as you sound the bell put out the light, and the subject declares he sees a green light. In other words the retina and the lower centers have been impressed and have performed their function of bringing out the complementary color, though the subject's mind declares itself utterly unconscious of the impression. M. Fontan also makes other apt suggestions on the nature of negative hallucinations or unconcious perceptions as he prefers to term them. (*Revue Scientifique*, May 11, 1889.)

In a preliminary communication in regard to the heat regulation of the human body, Loewy (*Pflüger's Archiv.* XLVI, p. 625) states that he has obtained the following results. The regulation is not perfect; the temperature of the body falls, without exception, upon the withdrawal of a large amount of heat, and in most individuals upon the withdrawal of a small amount. The skin is the principal organ for the regulation of the temperature; cold causes a reflex contraction of the skin and of its vessels, and hence a diminution of the heat dissipation. The heat-production remains unchanged (and hence plays no part in the regulation), so long as no muscular contractions take place. Muscular contractions can be inhibited, even when there is a very considerable fall in the temperature of the body. Extreme and lasting cold produces involuntary contractions. The increased decomposition produced by these contractions (trembling, etc.) is very considerable. It may reach 100 per cent. and probably much more. But still it is not sufficient to prevent a further fall of temperature. This means of regulation is not of practical importance, because we seek artificial sources of warmth long before it is called into play. It does not appear that Loewy made any investigation into the changes in the activity of the sweat-glands, as acted upon directly by their nerve-centers, and not indirectly by their blood-supply.

W. A. Turner, in a communication on the innervation of the muscles of the soft palate (*Jour. Anat. Phys.*, XXIII., 1889, p. 523), concludes: 1st, that there is not sufficient experimental or clinical evidence to support the doctrine that the muscles of the soft palate are supplied by the portio dura; 2nd, that experimental evidence shows that these muscles are innervated by the internal branch of the spinal accessory (*nervus accessorius vagi*), whose fibres are distributed along with certain branches of the vagus; 3d, that sufficient clinical evidence exists to prove that paralysis of the palate results from disease affecting the medullary center, the roots and the peripheral distribution of the vagus and its accessory nerve; 4th, that with the paralysis of the palate (*palatoplegia*) there is associated paralysis of the tongue (*glossoplegia*) and of the vocal cords (*laryngoplegia*), either unilateral or bilateral, according to the situation of the disease. F. T.

The profound effects following the emotional strain of great calamities has been frequently remarked. Those of the late Johnstown disaster are thus described. "Most of the faces that one meets, both male and female, are those of the most profound melancholia, associated with an almost absolute disregard of the future. The nervous system shows the strain it has borne by a tremulousness of the hand and the lip in man as well as in woman. This nervous state is further evidenced by a peculiar intonation of words, the persons speaking mechanically, while the voices of many rough-looking men are changed into such tremulous notes of so high a pitch as to make one imagine that a child on the verge of tears is speaking. Crying is so rare that your correspondent saw not a tear on any face in Johnstown, but the women that are left are haggard, with pinched features, and heavy, dark lines under their eyes. Indeed the evidence of systemic disturbance is so marked in almost every individual who was present at the time of the catastrophe that it is possible with the eye alone to separate the residents from those outside."

An interesting point of connection between passion and epilepsy (the resemblance of which was remarked by Echeverria) is noticed by Dr. Ch. Féré in the *Revue de l'Hypnotisme* for June, 1889. Observations on blood pressure during the aura show a rise of 200—300 gms. This high pressure continues through the convulsions, but gives place after the attack to one below the normal, which lasts 8–10 hours or longer, and in serial attacks even for several days. A rise of the same sort occurs in the attacks of mental excitement and agitation. Here cupping or mustard baths reduce it very materially (with a concurrent decrease of the symptoms), though the reduction which they effect is slight upon subjects in the normal condition. It is known that violent effort or emotion (both attended by increased pressure) may bring on the fits. One of Féré's cases, an epileptic with a normal pressure of 800 gms., showed 1050 in a period of attacks and 1100 in a fit of anger. An angry imbecile had a pressure of 1000–1100 against 850 when undisturbed; an angry cabman 1100 against 800.

In the *Medical and Surgical Reporter*, Feb. 9, 1889, Dr. Geo. M. Gould gives three cases, one of chorea, one of flatulent dyspepsia of twenty years standing, and one of palpitation of the heart, in which the trouble seems to have been reflexly caused by eye-strain, and ceased after the correction of the ocular errors. In the number of the same journal for March 9, he explains the alleged greater frequency with which eye-strain results in troubles of the eyes themselves before puberty, and in headaches and reflex neuroses after puberty, especially in women. This he does chiefly by reference to the general principle of sexual selection; reflex troubles are less of a hindrance in securing a mate than visible and disfiguring troubles of the eye would be, besides leaving an important organ free to perform its function. In the general causation of eye-strain, the near work required by present civilization is of course an important factor.

A case of epilepsy, described by Drs. Hughlings Jackson and Beevor before the London Medical Society (*Brit. Med. Jour.*, Feb. 23, 1889, p. 414), is of special interest, as going to support Ferrier's location of the olfactory center in the hippocampal lobule and adjacent parts. The fits were not severe, the patient neither fell nor lost consciousness, but as a part of her aura had an indescribably horrible smell. Post-mortem examination showed a tumor at the end of the tempero-sphenoidal lobe, surrounding the amygdaloid body and crowding upon the dentate nucleus and the fibers of the internal capsule; the cortex of the hippocampal convolution was uninjured.

The presence of an olfactory hallucination rather than anosmia shows the center to have been excited but not destroyed.

Prof. Venturi, whose study of temporary insanity was noticed in this JOURNAL (Vol. II, p. 178), has added a new case of temporary melancholia, (*Archivio italiano per le mal. nervose, etc.* 1888, XXV, 369). The patient was a temperate and industrious man, 32 years old, and previously of sound health and of sound heredity. After a not inexplicable fit of ill-humor (lasting from one day through the next, but not interfering with sleep or work), just after going to bed, he suddenly fell into the deepest melancholic agitation. On examination 3 hours later, he showed on the psychic side profound disturbance of consciousness, delirium of fear with visual illusions, mental distress, attempts at flight, though not at violence to himself or others, (there were thoughts of suicide at first), and on the physical side, dilated, but responsive pupils and cutaneous hyperæsthesia. Under morphine he quieted down, and after 3 hours' sleep awoke much improved. He slept again in the forenoon and by evening was given over as perfectly restored, though with incomplete recollection of the attack. The insanity did not outlast 14 hours. Dr. Sommer in abstracting the paper for the *Neurolog. Centralbl.* No. 8, 1889, remarks that while such cases rarely come under scientific observation, they are not really so rare as might be supposed. The city-asylums in a large city could undoubtedly furnish a good many examples.

The hold taken by scientific ideas of inebriety is shown by the character of the asylum just started by Prof. Forel of Zürich and Dr. Bleuler. There is a farm connected with the institution on which the patients labor. Total abstinence is enforced; labor is compulsory; the patients are received for a minimum period of six months; hypnotic suggestion is employed; and persons decidedly insane are not received.

The following questions for the discovery of those sensitive to "odic" force are taken from an account of the work of Reichenbach in the *Revue de l'hypnotisme*, Juin 1889. Most of them seem to be the generalizations of experience, and it is easy to see that one who could answer many of them in the affirmative would have the hypersensitive nervous organization and the general suggestibility that would make him a good subject for the demonstration of any occult phenomenon—"odic," or hypnotic.

1. Is your sleep calm or disturbed?—The sensitive generally has disturbed sleep.
2. Can you sleep two in a bed?
3. Do you not experience uneasiness when you find yourself in the midst of a crowd at the theatre or in a church?
4. When you give your right hand to any one is it indifferent to you that the pressure of the hand is prolonged?
5. When you handle pieces of money, or better of copper, is it indifferent whether you do so with your right hand or your left? Do you put the money in your right-hand pocket or in the left?
6. Do you experience any sensation when you look at yourself in a mirror? Is the sensation agreeable or disagreeable?
7. When you set the ends of your ten fingers lightly against a wall do you not after a while experience a sensation of heat in one hand, of cold in the other? Which of your hands is cold?

8. When you hold the palms of your hands one beside the other, and they are lightly blown upon is there a difference in the sensation felt by the two hands? Which of the hands has a sensation of heat, which experiences a sensation of cold?

9. Is it disagreeable to you to have flowers in your room at bedtime?

10. Passes made over your hand, your arm, your face, your body—are they felt?

11. Is the direction of these passes indifferent?

12. Do you like sweet and rich food? acid and salt? bitter and astringent?

13. Do you talk in your sleep?

14. Do you distinguish blue and yellow with reference to the impressions produced?

Finally, if it is desired to discover the degree of sensitiveness of the subject, it will be sufficient to make use of passes at a distance.

In a note in the *Revue de l'Hypnotisme* for June, 1889, Dr. E. Dupuy mentions a case of most unusual control over the ocular muscles. The person in question, a woman of 27, was able to move one eye in any of the ordinary planes of motion while she kept the other fixed. She used either eye at will for vision during these motions but not both, the attempt to do so giving rise to a sensation of falling. Her ordinary binocular vision appeared uninjured by these ocular gymnastics.

The following definition of a living being is given by Fernand Lataste in the *Comtes Rendus de la Soc. de Biologie, séance du 5 Jan.*, 1889: "A living being is a being composed of elements, in incessant chemical renewal and reacting upon one another in a way to maintain the form and functions [of the being] in a determined cycle of evolution, similar to the cycle traversed by other living beings from which the one under consideration comes forth or to which it is bound by community of origin."

PERISCOPE.

The remarkable awakening and concentration of interest upon psychological themes within the past few years indicates not only increased activity but a change of direction, if not even of base. More and better methods of both observation and experiment have yielded important results and promise far more important ones in the near future. The movement is peculiarly significant because it indicates that man is to attain a better and more objective knowledge of himself than ever before, and at the same time almost all valid results have immediate applicability in educational methods, mental hygiene, preventive medicine and moral and religious opinion.

It may interest our readers if we note a few important movements in the field represented by the AMERICAN JOURNAL OF PSYCHOLOGY within the two years of its existence completed by this issue.

Within this period falls the first international congress for criminal anthropology, held at Rome and organized by Lombroso, Ferri and others to discuss such questions as the following:—is there a general bio-pathological character predisposing to crime and can different sources and types of it be distinguished; is suicide in inverse relation to murder; moral insanity and epilepsy in prisons and insane asylums; classification of criminals; simulation and dissimulation. A large exposition of skulls, brains, photographs and graphic tables was opened in connection with this congress. While Italian psychology has made its most valuable contributions in this field much besides has been done. Sergi has digested and at several points amplified the work of the English school, making as the strange center of his system the idea of the pathological character of all religions. Golgi has made his remarkable contributions of new methods and results in the field of brain histology. Mosso, one of the most genial and talented of living physiologists, is devoting himself more and more to problems of a chiefly psychophysic character, while many younger men represent nearly all the modern tendencies to be found in Europe in the psychological field. Fresh modern research is taking the place of the speculative views, which far from holding their own in the rapid and new University development of that country, have steadily lost ground since the death of Vera.

In France the exposition has seen the formation of *L'association Internationale d'Hypnologie*, of which M. Bérillon was the leading spirit and the origin of the *Congrès internationale de psychologie expérimentale*, under a permanent committee of 26, and with a membership of nearly 400. The *Revue de l'Hypnotisme* has added to its name '*et de la psychologie physiologique*' and modified its contents accordingly. The school of suggestion has carried all before it in the study of hypnotism, (the therapeutic value of which is daily proven,) and has a new monthly organ *La Clinica*, edited by A. S. Herrero. In the *École pratique des Hautes Etudes*, Professor Beaunis has opened a new laboratory for physiological psychology in the building of the new Sorbonne. The *Société de psychologie physiologique* has now been established nearly two years, and began with thirty titular members residing in Paris and corresponding members in the provinces. Its work, so far as we can infer from its publications in the *Revue Philosophique*, is mainly upon phenomena which may be called abnormal, but not morbid, and promises to shed much light upon current popular delusions like spiritualism, telepathy and mind cure. Ribot has been made professor of physiological psychology, first in the Sorbonne and then in the College de France and has continued to make the most convenient and popular of all digests of the results obtained by German savants in both morbid and experimental psychology.

In England very little has been accomplished in empirical psychology, save the valuable anthropological work of Galton, and the work of Horsley and of Schäfer. *Mind* has more than held its own as a model of editorial care, and the Society for Psychical Research, without expressly relinquishing its telepathic theories, is gradually turning its energies into more fruitful and more promising channels.

In Germany hypnotism has made much and sure progress as a therapeutic agent, but almost solely on the basis of the theory of suggestion and chiefly among alienists, where observations are more painstaking than those made in France. The laboratory of Wundt has been much enlarged and now comprises half a dozen well filled rooms. Kraepelin at Dorpat, Preyer at Berlin and Krafft-Ebing at Vienna have begun labors in their new fields of work in lines represented by this JOURNAL. Münsterberg at Freiburg has commensed a very promising and comprehensive series of "contributions to experimental psychology," Ebbinghaus and Goldscheider at Berlin, G. E. Müller, the successor of Lotze at Göttingen, Monakow and Forel, Moll, Loeb, Dessoir, Edinger, Benedict and scores of others have made valuable contributions from their various standpoints. Noteworthy is the growing tendency to psychological lines of investigation by physiologists. This is seen not only in Pflüger's and Du Bois-Reymond's Archivs, but in Holmgren's new Scandinavian Archiv, in the work of Setschenow's pupils in Russia, particularly Orschansky at Charkow and among American physiologists, particularly in the valuable work of Bowditch of Boston. In Germany the history of philosophy seems settling into a standard university course desirable for all who can give time to it. Kant philology and the theory of knowledge are nearly exhausted lines of work, but even in these courses it is noteworthy that psychological subjects enter at every possible point and everywhere give zest. Psychology has in Germany already awakened the hopes and fears of religious leaders who see in it a great and rising movement. Even in Scandinavian universities the all pervading influence of Boström is yielding to the younger psychologists.

In this country Dr. Jastrow has established rooms for psychological research in the University of Wisconsin, where several determinations have been already made, which will be published in the next number of this JOURNAL. Dr. Cattell has a laboratory for psychological study in the University of Pennsylvania. Dr. Wolfe is introducing these studies into the University of Nebraska, and Dr. Baldwin has been called to represent them from his standpoint at the University of Toronto. Professor Ladd at Yale and Professor James at Harvard have both apparatus for demonstration and research in experimental psychology. In the faculty of Clark University Dr. F. Boas represents the psychological parts of anthropology, Dr. Donaldson neurology, including morbid psychology, and brain localization, Dr. Hall and Dr. Sanford experimental psychology, and Dr. Hodge, whose valuable work has been published in the JOURNAL, is a fellow. The *Anthropologist* and the *Folk-lore Journal*, both less than two years old, the establishment of an experimental station under the competent direction of Dr. Wm. Noyes at the McLean Asylum for the Insane at Somerville, and the valuable work of Dr. Cowles in interesting the American Association of Asylum Superintendents in experimental psychology, the work of the Seybert Commission, and of the American Society for Psychic Research, which is hardly less indebted than is this JOURNAL to the wealth and generosity of Mr. R. Pearsall Smith, the growing demand of our colleges for well trained psychologists, and, we may perhaps be permitted to add, the growing subscription list of this JOURNAL—these and many more indications promise new and better things in the study of the human faculties. In the number and scientific interest of its rare borderland neurological phenomena this country unhappily presents opportunities for certain lines of study unequalled elsewhere. Thus if we consider opportunity for observation, direct practical utility, the high degree of general interest and the wide inferences concerning the profoundest of all human interests which we expect eventually to see warranted in these fields, which we shall intimate later, we must conclude that we are now at the beginning of developments of great importance for science and for man.

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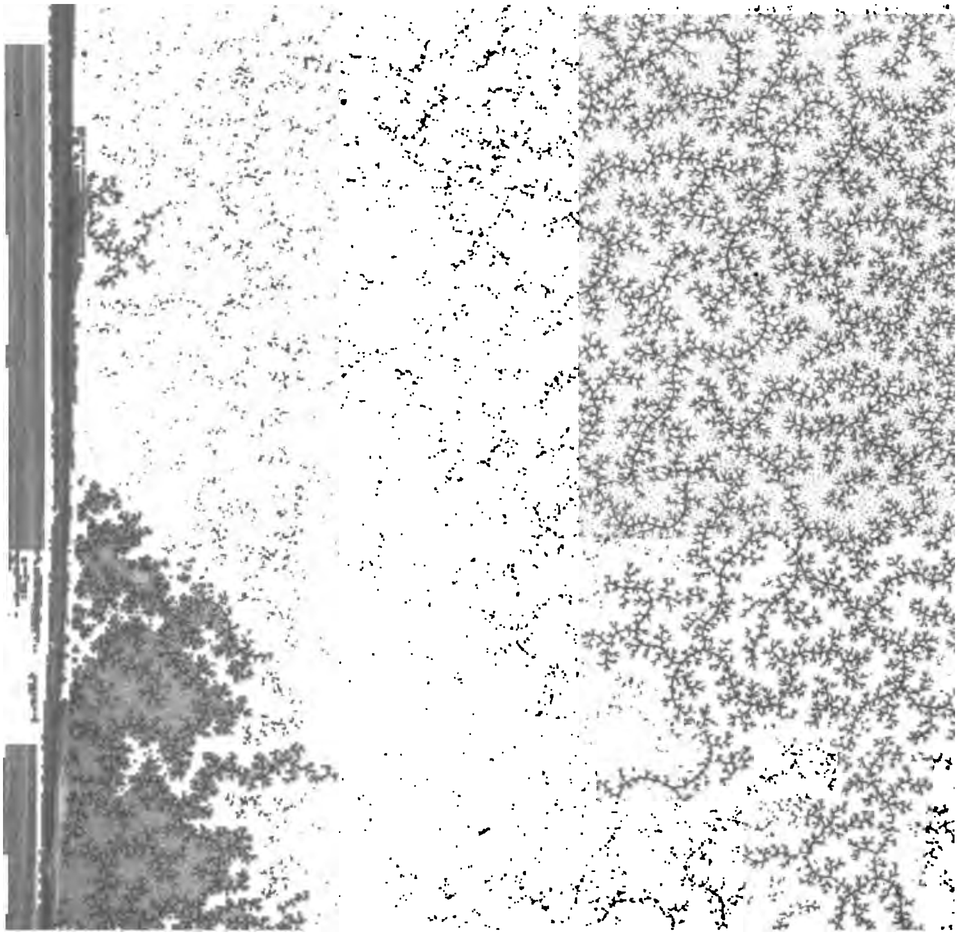

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